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RIDDLES WROUGHT IN IRON AND STEEL.

BY PAUL KREUZPOINTNER,
Altoona, Pennsylvania.

In view of our apparently extensive knowledge of the nature of iron and steel, it may seem strange to still speak about riddles wrought in these metals.

Nevertheless, in everyday practice we are constantly confronted by riddles of one kind or another, when dealing with iron and steel, particularly the latter.

One's ingenuity is sometimes taxed severely to find an explanation for the cause of such riddles.

We pour some fluid iron or steel into a mould, and what have we?

A metal liable to puzzle us with various kinds of often contradictory phenomena which may be of no consequence, or perhaps be annoying and sometimes hurtful.

We congratulate ourselves on having taken all precautions to insure a good casting, when lo! we hear a crack and

find the subject of our pride a useless mass of metal. We say it was caused by shrinkage.

Can any one describe the laws according to which the various elements of which the metal is composed aggregate to themselves the proper percentage of the most congenial elements at the proper degree of heat in the most suitable time and in such a position as to be torn apart *en masse* at a given point during the cooling of the mass, and what causes the casting to crack on account of shrinkage?

We know well enough how internal strains end, provided they end injuriously, if not disastrously, but who can solve the riddle of how they begin, how they progress, and how they stop just within, or far below, the point of rupture, as the case may be?

The riddle becomes no less puzzling if we roll that steel, if it was steel, into plates, use those plates for several years and then all at once some of them begin to crack lengthwise and crosswise and are so brittle that a blow with a hand hammer breaks off strips of the material. Yet that steel met all chemical requirements and physical specifications before the plates were put in service and after they had disastrously failed.

When some twelve years ago the author called the attention of a foreign steel-maker to the fact that soft steel was being used very successfully in this country for boilers, he said that would never do.

He had known of steel plates cracking while leaning against a wall. Now, is this not curious? Why should steel plates crack after they had passed rigid specifications? And why should there be greater success in the use of steel in one country than in another?

After you have solved this riddle I have another for you.

Why is it that one man softens a piece of steel by annealing, while another man anneals an identical piece of steel and finds it has become harder when his object was to get it softer?

Here are a few figures.

Wire with 49,200 pounds per square inch when unannealed was found to have a strength of 54,400 and 55,800 per

square inch after annealing for several minutes in a temperature of from 1,400° to 1,500° F.

In a series of twenty pieces of soft steel the author found not one of them softer after annealing. Two of the pieces were of the same strength and elongation as before annealing and all the others were harder and lower in elongation.

Why is it that we can raise the strength of soft staybolt iron of, say 47,000 pounds per square inch, to 60,000 pounds per square inch either by heat treatment, or by repeated application of stress?

Perhaps all of you have come across the man who knows it all.

If you meet him again please ask him to give the reasons why steel coming from the rolls or hammer is weaker, and less ductile, than the same steel is after left lying a day or two, or, better still, a week. Mr. A. A. Stevenson confirmed this here recently.

There is no doubt that many tons of suitable material have been either thrown out by the mill people themselves or were rejected by the inspector because it failed to meet specifications, causing needless vexation and friction simply because neither the one nor the other of the parties knew that steel is in a disturbed physical state after rolling or hammering, no matter how good the material, and should be left to rest, the longer the better.

Now, what takes place in the steel during the period of rest?

Another riddle is that we can raise the elastic limit and ultimate strength by a successive application of stresses very much above the original strength.

What law, if it is a law, governs this phenomenon?

Personally, the author is convinced that many errors of design or inherent weakness of the steel have been modified in their probable consequences, and breakdowns averted, by this pecular property of steel to gain in strength, if allowed to rest after having been subject to stresses within certain limits. It was the knowledge of this fact which enabled the author to fight for steel and defend steel for

structural purposes at a time when that metal was not yet a favorite with the engineer by any means. We are all familiar with Mr. Outerbridge's discovery of cast iron getting stronger by tumbling in the tumbling barrel, but for all we know it is still an unsolved riddle what the conditions really are producing such effects.

There is reason to believe that a similar phenomenon takes place in steel and, by analogy, in other cast metals. In looking over a table of the different degrees of temperature at which a certain number of plates were rolled, from the first pass to the last, I found one plate where the temperature remained 1,900° F. during three successive passes.

In another plate, an inch thick, the temperature likewise remained 1,900° F. during three successive passes.

In still another one the temperature oscillated between 2,100° and 2,200° F. during five successive passes.

Is this not a riddle?

When we look over the large field of results obtained by subjecting steel to strains of one kind or another we observe an almost endless variety of phenomena, which, on account of their frequent occurrence, may be familiar enough to us, but as to the reasons why, we are profoundly ignorant, showing us that our knowledge of steel is still rather fragmentary, notwithstanding the great strides we have made in the knowledge of the properties of that valuable metal.

If we heat a steel bar at one end, having it divided previously into inch lengths, and then break off the pieces, one after another, we will find the structure of the fifth or sixth piece very much different at both the fractures, though they are only 1 inch apart. Either one of the fractures is coarse-grained and the other very fine, or the one is granular and the other amorphous.

What does such a puzzling phenomenon teach us? Sometimes we are taught something which in after life we find not to be correct. Thus fifty years ago I was taught in school that elephants could not get up if they ever fell down. I was very much surprised when I saw an elephant for the first time and found he could lie down and get up like a horse.

We have likewise been told that elongation is proportionate to strength. That the one increases as the other decreases, and *vice versa*.

Everyday experience does not bear out this assertion. Looking over a list of 150 tests of steel of the axle grade, ranging from 74,000 to 103,000 pounds per square inch, an elongation is found of 27 per cent. at 76,000, 88,000 and 96,000 pounds strength. 20 per cent. is associated with every result from 80,000 to 99,000 pounds. 18 per cent. goes with 80,000 and 100,000 pounds, while 16 per cent. accompanies 85,000 and 103,000 pounds.

What causes this apparent anomaly?

Let us take the product of two different steel works, and if we test it in 8-inch section, the tensile strength and elongation may be found alike in both cases. Yet if we test the same steel with a 2- or 4-inch section we may find a difference of from 4,000 to 6,000 pounds per square inch, and several per cent. of elongation between the two products. Here we have not only a riddle, but a powerful argument why we need uniform methods of testing, and that it is in the interest of manufacturers as well as the engineer to bring about such uniformity.

Right in line with this puzzle is that other puzzle, shown first by Bauschinger and Wöhler, and confirmed since by numerous investigators, that removal of the load which is straining a piece of metal does not stop the activity of molecular motion in that piece, but that activity once aroused continues for days, weeks and months after the load has been removed. Thus, for instance, if a test-piece has been stretched somewhat beyond the limit of proportionality it will, on removal of the load, return partly to the original length at once. It will not stop there, however, but will continue to grow shorter for weeks and months, more rapidly at first, but slower and slower afterwards.

Having tried your patience with a recital of some of the riddles wrought in iron and steel across which the practical metallurgist stumbles every day, let us consider now some of the manifestations which apparently are the source of many of the riddles we come across.

To solve these riddles must be our constant endeavor if we are not to retrograde in the art of applied metallurgy and lose the advantages we have gained in our struggle for supremacy in the markets of the world.

One of the principal, though not the only, sources of the riddles we encounter in steel and other cast metals is, no doubt, that characteristic of alloys to segregate.

This tendency to segregate does not necessarily mean the formation of a nodule of same size within a shrinkage cavity of a chemical composition entirely different from the surrounding mass, or the lining of a shrinkage cavity with those pine-needlelike crystals so characteristic of iron alloys, nor yet of the formation of hard spots.

While all of these are common occurrences, yet we have to look for an explanation of some of the riddles wrought in steel to that kind of segregation which produces a chemical change in the whole mass of steel and makes itself felt by a change in the whole structure rather than in separate spots.

That eminent metallurgist, Freiherr Jüptner von Jornsdorf, in speaking of the problems we have to deal with in metallurgy, says:

"All accumulated experience points to the conclusion that in metals we are dealing with solutions, and that the various components are segregations, the nature of which depends, of course, on the various elements of which the solution is composed and the temperature at which these elements separate.

"That this process of separation on cooling varies according to the point of saturation of the solution is well known.

"In a concentrated solution a part of the dissolved elements segregate out at a decreasing temperature. On further cooling, still more of the elements segregate out, until, at a certain point of temperature and concentration, the remaining mother liquor, that is, the solvent and the dissolved elements solidify together without further segregation of the one or other element. * * * In practice this phenomenon may become complicated in so far that, for instance, a concentrated solution of the one or the other

element is present in the molten alloy. On cooling, the excess of this element segregates out, and the remainder, or mother liquor, represents a diluted solution.

"If now, on further cooling, part of the solvent segregates out, then it is conceivable that a reversal of the previous condition takes place, and the mother metal again becomes a concentrated solution."*

This short extract from that very interesting paper may give us an idea of the complexity of conditions and distribution of elements in cast iron and steel, due to their separating and segregating at different temperatures in ever-varying percentages.

Thus we can readily imagine the almost incomprehensible complexity of conditions, arising in a cooling mass of cast iron or steel, especially of cast iron, which is richest in divers elements, by the partial or complete absorption, saturation, equalization, segregation, exchange and interchange of the various elements, according to their affinity, the law of crystallization governing them, degree of melting heat and rate of cooling. In these reciprocal effects of absorption and segregation, of solution and saturation of the varying and ever variable elements composing cast iron and steel, complicated still more by the melting point and rate of cooling, we no doubt often may find the reasons for at least some of the riddles wrought in iron and steel. As we go down in the scale of temperatures from the highest degree of fluidity to the point of complete solidification we have a series of formations of groups of elements, the last of which decides the quality and usefulness of the metal.

Conversely, when heat is applied to a solidified mass of metal, the various groups of elements will not soften and melt simultaneously, but in succession, and herein we find the source of many riddles and complexities due to the heat treatment of steel.

It is, indeed, an indisputable fact that we can spoil a

* Die nächsten Aufgaben der chemischen Untersuchung von Metallen, besonders von Eisen und Stahl.—*Baumaterialienkunde*, Heft 5 and 6, Bd. II, 1897.

good piece of steel by improper heat treatment and can improve an inferior grade of steel by proper heat treatment, and we can also produce different qualities in the same piece of steel by a judicious heat treatment.

Here, for instance, is a series of four microphotographs, each of which shows a different structure, as well as a different tensile strength and ductility, although the four pieces are one and the same piece of steel, having had exactly the same structure and physical qualities and chemical elements originally.

The photographs were made for the Franklin Institute by Mr. F. D. Maisch, of this city, from the piece of steel treated, and are seventy times magnified.

Fig. 1 is the original steel. Its tensile strength was 103,000 pounds per square inch and 15 per cent. elongation in 2 inches. We see by the dark, dense blotches and irregular structure that the steel was in a disturbed condition.

The carbon was 0·4 per cent., and the high strength as well as the appearance and irregularity of structure for this grade of steel indicate internal strains.

The original piece having been cut into four pieces, piece No. 2 was heated to produce what the author considered normal axle steel. The result is seen in *Fig. 2*. The structure has become quite uniform and more open-grained than in *Fig. 1*. The tensile strength has fallen to 87,000 pounds per square inch, and 35 per cent. elongation in 2 inches, a decrease of 16,000 pounds per square inch and increase of 20 per cent. elongation.

A third piece was heated in a different, though not unusual way, when *Fig. 3* was obtained.

This brought the tensile strength down to 84,000 pounds per square inch and 22 per cent. elongation in 2 inches. The structure has become coarse, showing a distinct separation into two well-defined bodies of metal. The fall in strength and elongation indicate impaired quality of the steel, if we consider *Fig. 2* as the most suitable quality.

The fourth piece was then annealed to a high degree of heat and left to "soak" for two and one-half hours.

The result we see in *Fig. 4* when the strength had fallen

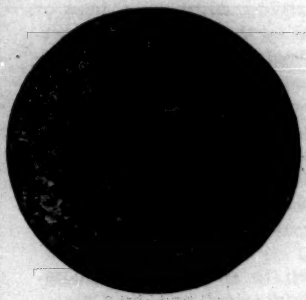


FIG. 1.

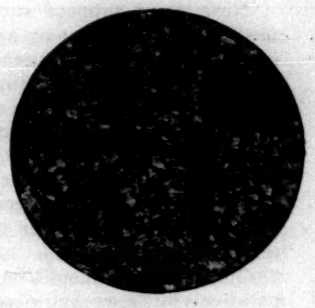


FIG. 2.

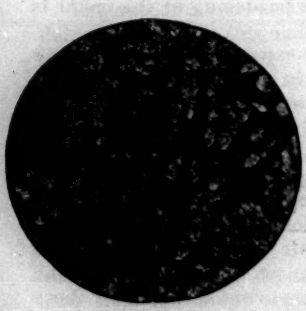


FIG. 3.

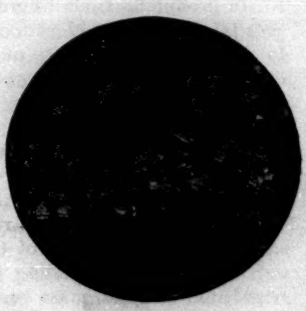


FIG. 4.

PHOTOMICROGRAPHS SHOWING CHANGES OF STRUCTURE IN SAME PIECE OF STEEL CAUSED BY DIFFERENT HEAT TREATMENT (× 70).

to 80,000 pounds per square inch and 20 per cent. elongation in 2 inches. The structure has changed radically, and clearly shows overheating.

Now let us suppose such varying degrees of heat or modifications of them are applied to the members composing a structure, or to the individual pieces of a lot or shipment, then we can readily imagine the almost endless variety of structures in the steel of perhaps a day's work, of comparatively uniform chemical composition, and, what is still more important, the ordinary specifications for tensile test may bring all these structures, good, bad and indifferent, within the limits of acceptance.

Do we need to wonder if we find riddles wrought into that metal?

Other instances could be given to show you how comparatively little we know yet about steel and how much there is yet to learn when we can produce at will a difference of 23,000 pounds per square inch of strength and 20 per cent. of elongation in the same piece of steel at the same time.

In another case, the author succeeded in producing a difference of 21,000 pounds.

In producing such differences hardening of the metal is not taken into consideration.

While our knowledge of the causes and effects of the action and reaction taking place in steel is as yet rather fragmentary, hence the riddles, yet we are beginning to grasp the underlying principles.

We know now that steel is an alloy in which a part of the elements is held in solution by the iron, while part is a mechanical mixture. We are beginning to learn that there is no stability between the relations of the various elements at each and every stage of the life of steel.

We are comprehending that iron and steel is not the rigid, immovable mass we were wont to consider these metals, that they are, in fact, like very sensitive creatures, liable to be influenced by changes of temperature, unwilling to be abused and ill-treated.

It is the degree of this changeability of steel which

to-day requires our closest attention and study, the condition under which it changes, the reasons why it changes and what changes make a given grade of steel suitable or unsuitable for a given purpose.

The want of this knowledge causes many phenomena to appear as riddles to us at present.

What we need to do now is to make use of the susceptibility of steel by changing its nature to our advantage in producing the most suitable quality, at one time, or, at another time, to secure greatest possible uniformity, not only in one piece, but in a number of pieces.

We must use the pyrometer more freely than we do now in order to be able to determine the proper temperature at which to anneal and to temper and to produce the same results uniformly at all times. We must discard thumb-rule and guesswork in the present treatment of steel, just as we have discarded thumb-rule and guesswork in mechanics.

We must study the microstructure of steel in its relation to physical qualities and chemical constituents.

We must find the best methods how to measure and determine the properties and qualities of steel.

The author is well aware of the practical difficulties of doing all this, but this should not hinder us from adapting our methods of testing and investigating the qualities of steel as closely to its nature as we know how and to the condition under which we use it.

At present we often follow the old beaten path without paying due attention to changes in economic conditions and our improved knowledge.

Some of the riddles we encounter in iron and steel are not riddles in the sense that they are indications of a perversity of these metals, but they are simply and solely due to our tendency to treat the physical properties of iron and steel as things which can be measured like their chemical constituents, in the chemist's balance, or conform themselves to every notion we may have about the manner and method in which these metals are finished, tested and worked.

Since we are beginning to learn what peculiar effects heat treatment has on steel, we must go a step further in our progress and determine the finishing heat, the tempering heat, the annealing heat, the quenching heat, with the aid of the pyrometer, for a given grade of steel. If we are satisfied with wide ranges of the qualities in steel, we may go on in the ways we have followed thus far.

But if we want to use our resources to greatest economical advantage and be tolerably sure what we are doing in order to be sure what we are getting, we must adapt the means to the end in proportion as we progress in our knowledge.

DISCUSSION.

MR. WM. R. WEBSTER:—No doubt Mr. Kreuzpointner could have given us very satisfactory explanations of his "Riddles Wrought in Iron and Steel," but preferred that they should be thoroughly discussed, and thus call attention to the necessity of improving the heat treatment of steel in its manufacture, and subsequent operations of working it in the shops, and the desirability of standard methods of testing. The points raised are well taken, and will, no doubt, bring out a full discussion and accomplish the ends aimed at.

Data bearing on many of the "riddles" can be found in the recent papers and discussion on the "Physics of Steel" before the American Institute of Mining Engineers. Segregation and its effects were then taken up, and the following is quoted from Mr. R. D. Hibbard's remarks:* "To guard against segregation in ingots the following precautions may be observed:

"(1) Cast ingots of the smallest practicable size.

"(2) Cast ingots of as cold steel as practicable.

"(3) Cast ingots as slowly as practicable.

"(4) If the ingots must be large, and segregation is very objectionable, then purer stock than is otherwise needed must be used, or allowance made to cut off from the top or out of the center, or both, enough steel to remove the parts containing the worst segregation.

Proc. Am. Inst. Min'g Engs., xxiv, 1894.

"(5) The method mentioned above, of making what I have called incipient rising-steel, must, it would seem, give ingots free from segregation. The steel in the mould is constantly in motion until the moment of solidification, and the impurities have no opportunity to collect anywhere. This method being applicable to soft steel only, however, will give no assistance in overcoming segregation in large high-quality hard-steel ingots."

I will endeavor to give solutions, based on the heat treatment of steel, to some of these "riddles." Of course, the chemical composition of the steel must not be overlooked, as on it, in connection with its heat treatment and mechanical work, the quality of the material depends.

Regarding plates that have failed after several years of service, it is always a difficult matter to get at all the facts:

(1) Concerning the conditions of rolling.

(2) The tests made on the plates.

(3) The abuse they may have received in shop treatment.

(4) The abuse they may have received in service.

After failures of such material, when the chemical analysis does not explain the trouble, physical tests, both before and after annealing, will often throw much light on the subject. The examinations by the microscope are also of the greatest assistance. The piece of boiler plate which I have here is a very simple explanation of failures of this kind. This plate is of open-hearth steel; it passed both mill and shop inspection for chemical requirements, tension test, per cent. of elongation, and per cent. of reduction; yet in forming in the bending rolls for a 60-inch shell it broke through one of the punched holes to the edge of plate. Further examinations were then made of the plate, and nothing in the chemical analysis or tension tests, taken from the plate close to the fracture, would account for the trouble. My attention being called to the matter, I suggested that both cold and quench bends be made from pieces cut from this plate. The former failed, as shown by this piece, and the latter bent down flat, as shown by this piece. This proved that the whole trouble was in finishing

the plate, in rolling, at too high a finishing temperature. Further investigation showed that the cold bends had been omitted in the original testing of the plate. I would call attention to the importance of always making these cold bends, not relying on the quench bends alone.

The changes referred to by Mr. Kreuzpointner as occurring in a piece of steel when heated at one end are shown in this piece of steel, it being a modification of Mr. Metcalf's beautiful experiment, and shows in one fracture all of the changes referred to, the difference in the sizes of the grain being a record of the temperatures to which the steel was heated. While we may not know the true explanation of this change, or when it takes place, yet we do know how to produce it, how to restore it, and how to avoid it. In the present case the large grain was produced by overheating the end and allowing it to cool slowly; it can be restored by careful heat treatment in annealing; if work had been put on this piece of steel while it was still hot, and continued at a low enough temperature, the large grain would have been broken up and a fine-grained steel produced. All steels undergo these changes. Their susceptibility increases with the increase of carbon.

The physical results from steel of any given chemical composition depend, to a large extent, on the finishing temperature. In axles, this accounts for the same per cent. of elongation for different ultimate strengths, and for the different elongations for the same ultimate strengths. The tendency of too high finishing temperature is to reduce the ultimate strength and per cent. of elongation, while too low a finishing temperature tends to increase the ultimate strength and reduce the per cent. of elongation. A definite solution of this "riddle" could have been obtained by carefully annealing the axles and making additional tension tests. The changes produced by annealing depend upon the chemical composition of the steel and the former treatment it has received, as well as the temperature to which the steel is heated, time kept at that temperature, and manner of cooling. In all cases the steel should be allowed to cool before it is heated for annealing.

The temperature of steel remaining the same, or increasing for successive passes, is accounted for by the heavy reductions, in rolling, elongating the material, and bringing nearer to the surface quickly the hot interior of the slab; and by the heat generated in this work. The extent to which the heat generated by work can be carried is cited by Mr. Metcalf in his "Manual for Steel Users," in which he states: "A skilful hammerman will take a piece of mild, cold steel, and by means of light, rapid blows he will heat it up to a bright lemon heat without fracturing it; then he will have it thoroughly plastic and malleable."

The changes of structure noted above occur in wrought iron. I have known of iron eyebars being condemned for a coarse crystalline structure in the neck, no work having been put on the bar at that point when it was heated to form the head. Some of the manufacturers, in order to guard against this, annealed all of their iron eyebars. I further recall one lot of upset iron anchor bolts that on testing, full size, broke at the base of upset, showing a coarse crystalline structure, and after carefully annealing broke in the body of the bolt, showing a tough fibrous structure.

I have only referred to the direct effect of the heat treatment on the physical properties of the steel. Its effect on the form of the carbon present should not be overlooked in this discussion.

In considering heat treatment, its effect on cast iron must not be overlooked, as many of the changes in the structure of this metal are due to heat, and are closely related to the corresponding changes in steel. This is shown in many ways; for instance, from the same ladle of iron a fine grain is produced in a small casting and a large grain in a large casting, the difference depending on the composition of the metal to start with, temperature of metal when poured and difference between the sizes of the castings. Another way of stating this is that the grain in all cases depends on the chemical composition of the iron and its heat treatment.

The large casting holds the heat much longer and allows the large grain to form. This is proven by producing the

large grain in the small casting by keeping it at a high temperature and delaying the rate of cooling.

Those are the direct effects of heat, but there are also the corresponding changes in the form of carbon present.

One will naturally ask: can corresponding changes be made in iron castings, after they have cooled, by a simple heat treatment that does not change the total amount of the elements present?

This, no doubt, will be answered by Mr. Outerbridge and others who have had a great deal of experience in this line of work.

I would like to ask the following questions:

(1) Can white iron castings be changed to good gray iron by annealing? If so, what is the gain in strength over corresponding gray iron castings produced in the ordinary way?

(2) I would also like to know what changes can be produced in good gray iron castings by heat treatment alone.

In these questions I do not refer to any changes depending upon the total amount of carbon or other element present, for instance, by treatment with oxide of iron or other material, but merely to simple annealing (as generally understood for steel), in the first case, and heating and quenching in the second.

A. E. OUTERBRIDGE, JR.:—The letter I received a few days ago from the Secretary of the Institute, relating to the special meeting called to discuss Mr. Kreuzpointner's paper, requested me "to take up such points as deal with cast iron and the behavior of molten metal."

There are riddles in cast iron as well as in steel, some of which will probably remain unsolved as long as the metal shall continue to be used, and I purpose making some statements regarding the behavior of cast iron when subjected to heat treatment which are, I believe, at variance with the current views, and are, therefore, open to criticism and correction should they be found not in accord with actual facts. I may say, in the first place, that there is a close analogy between the behavior of cast iron and steel, in some respects, when subjected to heat treatment, but in others there are much more radical differences of behavior

than our intimate knowledge of the composition of cast iron and steel would lead us to suspect.

Let us cite an instance where there is, evidently, a close analogy between cast iron and steel. If, for example, a bar of steel is heated to a certain temperature and suddenly quenched in cold water, it will become hard and more or less brittle. In like manner, if a bar of ordinary gray cast iron be heated to a certain degree (which may be called the "critical temperature") and suddenly cooled by immersion in a chemical solution of various salts, or even in cold water, it will become extremely hard, not merely upon the surface, but throughout the mass. I should, perhaps, explain that I do not now refer to so-called "chilled iron," *i. e.*, a metal in which the carbon and iron are in chemical union, having a characteristic crystalline structure and white color.

It is possible, in this simple manner, to change a comparatively soft gray iron casting into a metal as hard as steel; in fact, cutting tools for lathes and planers have been made of common gray cast iron thus hardened, not "chilled" on the cutting edge. The hardened bar of cast iron is very brittle and the fracture is much lighter in color than the original metal, closely resembling tool steel. Singularly enough, this fact does not seem to be universally known, yet this is one of the few instances in which cast iron and steel behave alike under similar conditions. But you may say, "What about the strengthening of cast iron by annealing; is there not a close analogy in this respect to steel?" I answer emphatically "No!"

It is, of course, very well known that some castings, such as chilled cast-iron car wheels, are greatly improved in strength by annealing in heated pits for a period of from three to five days, and from this fact it is generally assumed that annealing cast iron strengthens the metal. Nevertheless, I am prepared to make the positive statement—which may seem startling in view of the widespread belief to the contrary—that annealing gray cast iron by heat *invariably weakens the material.* How, then, can we explain the apparent anomaly that a cast-iron car wheel is strengthened by

heat annealing, if it be true that gray cast iron is invariably weakened by heat annealing? This is a riddle that may be readily solved.

A chilled cast-iron car wheel is subject to abnormal cooling strains due to the conditions under which it is cast. If a car wheel is allowed to cool in the air, like an ordinary casting, the cooling strains are sufficient to overcome the molecular cohesion of the metal and usually to cause the wheel to break asunder with a loud report; but if it is placed, while still red-hot, in an annealing pit and allowed to cool very slowly, these strains are gradually eliminated and the casting then becomes exceedingly strong. If now you ask, "Is the gray metal of which the plate of the wheel is made as strong after annealing as before?" I say, positively, it is not. Such a statement, however, to be convincing and to have practical value, must be accompanied by irrefragable proofs. Several years ago I made many experiments to ascertain the facts in the case, and very recently I have repeated some of these tests with entirely corroborative results.

Several bars of cast iron of various grades, ranging from soft pulley metal, having a tensile strength of about 18,000 or 20,000 pounds per square inch, up to very strong iron showing over 40,000 pounds tensile strength to the square inch (on bars of 1-inch section cast 30 inches long), have been made and tested, both before and after annealing, within the past few weeks. The *modus operandi* was as follows: The bars were first broken once upon a testing machine with supports 12 inches apart and the transverse strength of the unannealed metal was thus ascertained; one of the pieces of each bar was then turned in a lathe and pulled on a testing machine, thus the tensile strength of the unannealed bar was also determined. The remaining pieces of the bars were then placed in the annealing furnace and subjected to high heat for nine hours; the bars were then very slowly cooled while still enclosed in the annealing cases to protect them from the oxidizing effect of the air. The annealed pieces were then treated in precisely the same manner as were the unannealed pieces of

the same bars to ascertain the transverse and tensile strength. In every case the annealed bars were much weaker than the unannealed portions of the same bars. Two recent tests will suffice to illustrate this. Two bars 30 inches x 1 inch x 1 inch were poured from strong cast iron taken from a ladle holding about 8,000 pounds of molten metal and the following records obtained:

Transverse strength of bars not annealed.

No. 1 = 3,350 pounds (center load).
No. 2 = 3,300 " " "
Deflection No. 1 = .1444 inch; No. 2 = .1278 inch.

Transverse strength (same bars), annealed.

No. 1 = 2,700 pounds (center load).
No. 2 = 2,700 " " "
Deflection No. 1 = .1556 inch; No. 2 = .1333 inch.

Tensile strength (same bars), not annealed.

No. 1 = 37,285 pounds per square inch.
No. 2 = 36,072 " " " "

Tensile strength (same bars), annealed.

No. 1 = 24,500 pounds per square inch.
No. 2 = 24,500 " " " "

The fracture of the annealed bars shows much darker gray and somewhat coarser grain than the unannealed pieces of the same bars; this is due to the change of combined carbon to the graphitic form, and accounts, in part at least, for the weakening effect of annealing cast iron.

Not wishing to extend this discussion to an undue length, I will allude to but two other radical differences between cast iron and cast steel. There is comparatively little difference in the physical characteristics of the metal in a large or in a small casting poured from one ladle of molten steel, but there is a great difference in the character of gray cast iron when poured from one ladle into large and small castings. Furthermore, there is a lack of homogeneity in different parts of the same casting, causing wide variations in the physical properties of the iron. This was proved in a conclusive manner in some tests made twelve years ago, a brief account of which first appeared in print in the *Iron Trade*

Review, November 25, 1897, and as this article (written by the editor of that journal) also gave my views upon the effect of molecular vibration, or shock, on cast iron and steel, to which subject Mr. Kreuzpointner has alluded, I will close my remarks with a reference to the paper in the *Iron Trade Review* of the date mentioned.

"Reference was made in our issue of the 11th inst. to the report of the Committee of Science and Arts of the Franklin Institute, on the discoveries of Alex. E. Outerbridge, Jr., of Philadelphia, concerning the effect of shock or vibrations on cast iron. A table of tests appended to the report gives the results of independent experiments conducted by the committee. In some cases the gain in strength in bars tested by the committee by rumbling exceeded the maximum gain published in Mr. Outerbridge's tables, by more than 100 per cent. The largest gain claimed in Mr. Outerbridge's paper read at the Pittsburg meeting of the American Institute of Mining Engineers in February, 1896, was a little less than 19 per cent. The Franklin Institute Committee made its own bars and tests and obtained 40 per cent. gain in strength and 48 per cent. in deflection, as a maximum increase in the strength of a bar after it had been subjected to repeated shocks. In some cases bars which had been subjected to shocks in the barrel were found to be defective castings, yet they were stronger than companion pieces, free from defects, but not subjected to this treatment.

"The question has been raised whether a similar effect would be produced upon steel. Writing on this subject to the *Iron Trade Review*, Mr. Outerbridge says:

"'I have been frequently asked this question. I have always replied, 'No.' The conditions are radically different. 'Cooling strains' in cast iron must not be confounded with contraction of steel. The molecular structure of cast iron is determined by the rate of cooling quite as much as it is by the chemical constituents. A large casting and a small one from the same ladle of cast iron are unlike; with steel, however, no such great difference exists between large and small castings poured from one ladle.

"'Several years ago I had a solid block of cast iron 15 x 15 x 15 inches cut up into sixty-four test bars 1 inch square and 14 inches long, numbered and broken. There were eight rows of bars with eight bars in each row. The bars from the center of the block were coarse, open-grained metal, and the strength was scarcely more than one-half the strength of the bars from the outer section. There was a difference of nearly 40 pounds to the cubic foot in weight of the metal, as determined by specific gravity, between the comparatively close-grained, strong bars from the outer section and the open-grained bars from the interior. This explains, in part, at least, the tremendous cooling strains in cast iron, irrespective of the actual strength of metal, which can be relieved either by slow cooling, as in annealing a car wheel, or by continued vibrations of the metal while cold.

"'The large test block referred to above was made in 1888. The result of tabulating the breaking strains of the great number of bars cut from the block convinced me that the attempt to draw any conclusions regarding cast iron, as ordinarily employed, from the breaking strength, shrinkage, etc., of small bars, which are practically uniform in texture, or molecular structure—owing to quick cooling of the metal—would certainly lead to grave errors. Abundant proof of this has since been given to the world in various publications, and more, doubtless, is still to come.'"—*Iron Trade Review*, November 25, 1897.

In the foregoing remarks I have referred to the effect of heat treatment on gray iron exclusively, but Mr. Webster asks, "Can white iron castings be changed to good gray iron by annealing? If so, what is the gain in strength over corresponding gray iron castings produced in the ordinary way?"

In answer to this I would say that about twenty years ago the curious discovery was made that white iron, forming the hard wearing surface, or "tread," of some chilled cast-iron car wheels which had been accidentally overheated in an annealing pit, had been completely changed to soft, fine-grained, dark gray iron, by the heat treatment. No practical use was made of this discovery at

that time, but recently the principle has been applied commercially, with success, to the production of very strong gray iron castings. These castings are made originally of white iron, subsequently changed to the gray condition by annealing, and tests made by Riehlé Bros. of this converted gray cast iron show remarkably high tensile strength, ranging from 50,000 to 67,150 pounds per square inch. Axes, hatchets and other cutting tools are now made of white cast iron, then annealed to change them to gray iron (without decarbonizing, as in the malleableizing process) and subsequently hardened, on the cutting edge, by heating and sudden cooling. Such tools are now sold under the name of "steel castings," but this is, I think, clearly a misnomer. The metal is not steel, neither is it malleable iron, and it may be defined as a new commercial product standing between cast iron and steel, partaking somewhat of the qualities of each. In this process the heat treatment, or annealing, has changed a hard, brittle, white cast iron, only fit for sash weights, into an exceedinglys trong, fine-grained machinable metal, suitable for many valuable purposes and for which I think there is a wide field of usefulness in the future.

In reply to still another question of Mr. Webster, I would say that the grain, or texture, of gray iron in small castings may be very materially altered by slow cooling. For example, I have found by actual tests that light castings of thin section, made in a mould with a large casting, in close proximity thereto, will have more open grain and will be much softer than similar light castings poured in a separate mould from the same iron. There are other ways in which the same effect can be produced by prolonging the cooling of the metal in small castings, and in this way the necessity for annealing such light castings to make them more readily machinable may perhaps be obviated.

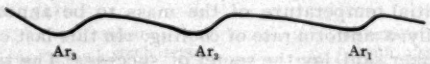
MR. G. H. CLAMER:—Modern metallography is rapidly solving the mysteries presented in the behavior of iron, steel and other alloys. By the aid of the microscope many facts have been discovered which chemistry could not account for. All iron carbon alloys show peculiar behaviors

under the influence of heat, and, if allowed to cool from a point above the melting point, will show at least one so-called critical point, at which degree of temperature a wonderful change takes place in the interchange of the contained elements or in their structural arrangement. For instance, very mild steel will show at least three such critical points, each indicative of an important change going on within the metal. A curve representing these changes would be as shown in *Fig. 1*.

A medium hard steel shows two such stops in the cooling curve, and the high carbon steels but one.

All steels, according to modern micrographic researches, are made up of at least two of the four primary structural components, which have been termed ferrite, pearlyte, cementite and martinsite.

Ferrite is iron free from carbon, but not necessarily free from other elements.



Pearlyte is a mechanical mixture or solid solution of ferrite and cementite.

Cementite is the carbide of iron Fe_3C. when it exists in segregated masses.

Martinsite—the true nature of which is not known—constitutes the hardening element in quenched steels.

When steel has been heated above the critical range, the constituents arrange, or perhaps combine, to form martinsite, and, if it be quenched before cooling down to the critical range, the point Ar_1 disappears altogether. On the other hand, all steels which have been slowly cooled below the critical range contain pearlyte and no martinsite. Steels above ·9 carbon (the saturation point of carbon and iron) contain cementite, whether quenched above Ar_1 or slowly cooled, the quenched steels consisting of martinsite and cementite, and the slowly cooled pieces, pearlyte and cementite; but never can pearlyte and martinsite occur together.

Steels below ·9 carbon, slowly cooled, consist of ferrite and pearlyte, whilst above ·9 carbon consist of pearlyte and cementite. Ferrite and cementite can never occur together.

With a knowledge of the above facts, which have established a basis for modern metallographic study of iron and steel, and the use of the pyrometer, I think many of the riddles which Mr. Kreuzpointner has pointed out could be satisfactorily solved.

MR. FRANCIS SCHUMANN called attention to the radical changes that had been introduced in modern practice in the annealing of glass. By the old method the time required for annealing large masses of glass varied from 56 to as much as 72 hours, and the results were not at all certain. By the modern methods lately introduced the annealing time is reduced to 45 minutes, and, with reasonable care, the results are uniformly good.

The conditions to be observed, he stated, are a tolerably high initial temperature of the mass to be annealed, and, especially, a uniform rate of cooling. In this last condition, the speaker said, lay the secret of success. The time occupied in cooling is found to be of little importance in affecting the quality of the results, but, whatever be the rapidity of the operation, the rate at which the successive decrements of heat take place must be as nearly absolutely uniform as possible to insure a successful result.

MR. ASA W. WHITNEY:—My experience has been in the regulation of mixtures for cast iron from the cupola, crucible and air furnace. The few problems to which I am prepared to direct your attention do not appear at first sight to have any bearing on the riddles of steel with which Mr. Kreuzpointner's paper is principally concerned, but when the great range of compositions included under the term Cast Iron is as fully investigated as is now the different range called Steel, these riddles, or problems with apparently insufficient data for their solution, will doubtless be solved as well as others have been for immediately practical purposes, thereby establishing more complete relations between cast iron and steel.

Referring to the weakening of the *metal* of cast-iron wheels by the annealing which strengthens the *wheels*, my similar tests on wheel iron bars some years ago confirm Mr. Outerbridge's statement. Moreover, the difference of ·05 to ·07 inch per foot between the contraction of the solid white chilled iron and of the solid gray iron of the casting gives some idea of the enormous strain which must be accommodated.

I have here a small specimen representing a class of phenomena in cast iron upon which Mr. Kreuzpointner's quotation on page 6 from Freiher Jüptner von Jornsdorf evidently bears. A casting from a mixture intended for soft light work, in this case about $\frac{5}{8}$ inch square, has a perfectly white core about $\frac{7}{16}$ inch square surrounded by a close, gray, moderately soft iron, the demarcation being very sharp. I have no analysis of this piece, but only recently Mr. W. J. Keep gives the following partial analysis of a very similar case ("Stratified Cast Iron," in *The Foundry*, December, 1899, p. 154):

	Gray Part.	White Part.
Total Carbon,	3·628 { comb. 1·754 / graph. 1·874	3·861 { comb. 2·554 / graph. 1·307
Silicon . . .	2·846	2·742
Manganese . .	—	·501

In neither case are the phosphorus and the sulphur given, and the manganese is given only in the white part. Had the figures for these elements been furnished, the percentage of iron could then be found by "difference," as usual, for lack of a more exact method. Then there would have been something to figure upon, and we would have had a problem instead of a riddle. But as it stands, it is not a fair question or even a legitimate riddle, and certainly does not warrant Mr. Keep's answer, "The chemical analysis of an iron will not indicate its physical quality, and nothing will, except a physical test." I believe that study on a few complete analyses of such cases would soon discredit such off-hand statements, and show how a very small calculated change on the composition of the charged mixture would obviate such a trouble in castings in cases where a change of melting conditions would not be feasible.

In my contribution to the subject of cast iron in the Franklin Institute *Journal*, April, 1897, I gave some instances of this phenomenon in charcoal pig iron, with full analyses and a photogravure. The silicon in these cases was below ·2 per cent. in both the white and the gray parts. I also referred to its occurrence with silicon, about 2·5 per cent. In Mr. Keep's instance it is higher. What study I have put on this matter is in the relation of the iron and the carbon; with the result, so far, that in the white part the ratio, or possible compound, Fe_3C is more nearly approached than in the gray part in the same pig.

Mr. Keep's crucible remelt of the white part alone yielded a casting of light gray fracture. My crucible remelt of the whole low silicon pig made a casting with a far larger white and solid core, surrounded by gray iron, even in a ½-inch section. Having in mind the carbon and iron relation and the total oxidation, these apparently divergent results probably correspond.

The case of reversal of ordinary phenomena just cited is really but a step beyond this other case illustrated by these two solid white chilled iron samples of the same size on the table before you. One is finely crystalline, and may be easily drilled, while the other is nearly as coarse and fully as hard as spiegel, and from appearance iron experts have supposed its contents to be from 5 per cent. to 15 per cent. of manganese. Yet the manganese in the hard sample is but ·8 per cent., and in the soft one ·35 per cent., and the difference in other elements is also correspondingly small. This result was intentionally produced, just to show the great difference that very small but properly calculated variations on melting charges will effect, the other conditions being constant. A softer chill of another character could also be produced by other changes, the manganese remaining at ·8 per cent.

Just to indicate that many of the so-called riddles which are still under discussion in cast iron have already been commercially solved, I show here one of thirty-three sheets of a large graphical tabulation, arranged by Mr. Wm. R. Webster, in 1896, of physical and chemical results on test

bars of wheel metal of 4 square inches sectional area and 12 inches long. In the eighteen tests on this sheet, representing as many different heats, the variation in transverse strength is but 1·5 per cent. either way from the average of 58,500 pounds modulus of rupture (or 3,260 pounds on a 1-inch square bar 12 inches long). The variation in contraction is 7·5 per cent., and in resilience 12·3 per cent. from their respective averages. But the lines showing the amounts and variation of iron, carbon, silicon, manganese, phosphorus and sulphur seem to be wholly irregular, with the exception of the line joining the amounts of iron in these eighteen bars. This line shows an approximate relation to the strength. Had this tabulation been based upon the best results of mixtures made in the old way by reference to the physical tests of a small range of metal, some such range of chemical analyses would have resulted and no further argument would have been thought necessary to discourage any attempt to control mixtures by calculations on chemical analyses of a wide range of material of which no physical tests would be known.

But the latter method, including reference to melting conditions and to the physical tests of previous heats, and very rarely to analyses of the cast metal, has given me success in a great variety of mixtures ever since 1889. What is often referred to as a riddle in cast iron, namely, the fact that practically equivalent castings do not show the same analyses, has in my practice resolved itself into the almost daily problem of producing practically equivalent castings by so adjusting the melting charges by means of chemical calculation that a different but suitable chemical composition will result in the casting. This is far more practicable than to attempt to retain one composition with sufficient exactness in spite of changes in stock, and looked-for modifications of melting conditions. Though the method is often very laborious, the results exceed in certainty, quality and economy, those of the old method which attempts to avoid riddles and chooses to pay for being consumed by them.

MR. H. V. WILLE:—No doubt, as Mr. Webster has stated, Mr. Kreuzpointner himself could have solved the riddles enumerated in his paper, but it is nevertheless unfortunate that the title "riddles wrought in iron and steel" should have been selected. Such a title can only serve to perpetuate the erroneous idea of the mysterious nature of some of the failures of steel, the causes of which are well known; and such theories can only serve to discredit the most useful product of the century. It is only too true that theories of this nature have existed, and it is not surprising when we review the development of the iron industries with special reference to the determination of the properties of the product. Makers of iron became makers of steel, and it is natural that they should apply the same methods for gauging the qualities of the new product that were applied to the old, namely, tension and bending tests. It was not long before they learned that, in place of globules of pure iron incased in a covering of slag, they were dealing with a complex chemical compound, and it was found necessary to apply an additional measure of the quality of the product, namely, the determination of the chemical properties.

During recent years the size of forgings and thickness of plates have greatly increased, and the finishing temperature of the plates and forgings is consequently much above the average range of temperatures obtained on thinner and smaller plates, so that manufacturers and consumers are now learning that in the physical and chemical tests they only have two-thirds of the story, and that it is impossible to solve many of the metallurgical problems that come before them on these insufficient data. They are, therefore, confronted with the necessity of the establishment of microscopical laboratories. When such laboratories become more general and we have (1) the physical properties of steel as shown by tensile strength, elongation and reduction of area; (2) the chemical properties as shown by the analysis; (3) the structure as shown by the microscope, we will then have data that will enable us to trace the whole history of a piece of material, and we will be no longer confronted with such riddles as Mr. Kreuzpointner enumerates.

A plea has been made for uniform methods of testing. This is all very well, but for the purpose of solving "riddles" it is much more important to have a uniform system of records of tests. The larger number of steel mills keep an entirely separate record of their chemical and physical tests. If these two records were kept in parallel column it would be very much easier to trace a relation between the chemistry and physics of the steel. Certain results in tensile strength, elongation and reduction of area, when considered alone, may appear to be perfectly normal, but may be quite abnormal when considered in connection with the chemical analysis and dimensions of the piece from which the specimen was cut.

To illustrate the value of such a system, it is necessary to cite but one instance of its advantage. There is one manufacturing establishment that does not believe in "riddles," but insists that all failures in boiler plates are due to defective material or improper manipulation in the shop. If the material be defective, the plate is returned to the manufacturer; if the workmanship be at fault, the plate is charged to the workman. At the end of the first year, when the work was being summarized, it was noticed that 90 per cent. of the plates that failed in flanging had an extremely low manganese content, usually associated, in basic steel, with low phosphorus, and the cause of the trouble, namely, oxidation of the steel after the phosphorus had been almost completely eliminated, was at once suggested; the low manganese being due to the utilization of a large portion of this element in the partial reduction of the oxide of iron formed.

The necessity of annealing has been referred to by several speakers. On this phase of the subject it may be well to point out that general and indiscriminate annealing may do much more harm than good, because, unless the annealing is carried on with proper safeguards, and done under trained supervision, the danger of overheating or of improper cooling is greatly enhanced. The annealing temperature should be predetermined for each grade of steel, and a self-registering pyrometer should be used, so that if this temperature be exceeded those in charge will know. Unless

these safeguards are used it would be much better to dispense entirely with annealing, because the temperature of the piece as it leaves the hammer is more likely to be nearer to the desired temperature of a hit-or-miss annealing furnace. In addition to this, if the temperature of the annealing furnace be too high, usually a number of pieces will be injured, while if the finishing temperature be too high, usually only one piece will be injured. For these reasons, in the present state of the art, I believe that the unannealed product of the average forge is really, in general, better than the annealed.

The same statements apply in a modified form to castings. An instance recently came under my notice of cast-steel wheel centers that had been annealed.

A test piece of about 1 square inch area was attached to a spoke of about 8 square inches. When the test was cut off and pulled it gave an extremely low elongation, a fracture of a mealy appearance and the section cracked in various portions like dried cheese; but a test piece cut from the spoke itself gave a good elongation and a silky fracture. Here we have an instance of the small section of metal being overheated in annealing, whereas the large section was properly heated. This is an extreme instance, but it illustrates the danger one runs and the care that must be exercised in annealing complicated castings.

MR. KREUZPOINTNER:—The author regrets having been prevented by illness to personally take part in the discussion of his paper.

However, he desires to thank Mr. Webster for his confidence by saying the author could have given satisfactory explanation of his "Riddles Wrought in Iron and Steel." Of some of the riddles mentioned he might have been able to do so, of others he could neither give a satisfactory nor any explanation at all.

And because the author feels that he does not yet know all about iron and steel he put his paper in such a shape in order to give opportunity to others, better fitted in every respect than the author is, to enlighten the members of the Mining and Metallurgical Section and not hide their light under a bushel, as it were.

Mr. Webster very properly gives particular emphasis to the effects of heat treatment of iron and steel, especially the latter, and the writer cheerfully supports Mr. Webster by saying that, other things being equal, the best steel, of whatever grade, can be made, and only too often is made, an inferior article by improper heat treatment, while on the other hand an inferior steel can be made quite acceptable by proper heat treatment.

The reasons why are a riddle to the author, like many other phenomena about steel, however well acquainted we may be with the fact.

Whenever heat treatment of steel and its effects receive that careful attention which the importance of the subject deserves then there will be fewer riddles to be solved.

The author has followed with particular interest the remarks of Mr. Whitney, whose extensive metallurgical knowledge and experience give weight to whatever he says on the subject.

Mr. Whitney rightly perceives in segregation the disturbing factor which modifies, and too often nullifies, the best efforts of the moulder and founder. Not a few unsolved and unsolvable riddles in metallurgy are due to this phenomenon in a fluid mass of an alloy, like cast iron and steel, for its component parts to break up into groups on cooling, thus disturbing the homogeneity, and hence the uniformity of structure of a metal.

This subject of segregation offers a fertile field for study and experimentation.

Mr. Wille may be right in wishing the author to have selected another title for his paper. However, as long as engineering societies and scientific bodies find it profitable and necessary to discover and discuss the why and wherefore of certain metallurgical phenomena which persist in forcing themselves upon the engineer's and steelmaker's and founder's attention, often to their sorrow, the choice of title does not amount to much.

Steel is recognized nowadays as such a valuable and indispensable metal by the engineer that neither the few cocksure nor the excessively timid can affect its standing in the future.

The author has fought for steel, defended steel, pointed out its valuable properties and qualities and predicted its triumph over wrought iron many years ago at a time when many of its present admirers were either doubtful or directly opposed to the use of steel for purposes for which nothing else is used now.

Yet, notwithstanding there is hardly an engineer nowadays who would not use steel as a structural material for most purposes, we still have to learn a good deal about its properties and qualities, as Mr. Wille rightly points out, and the ever-increasing variety of purposes for which steel is used, and new or changed methods of production, forever opens new opportunities for necessary study and investigation, with consequent discussion.

Mr. Clamer's enthusiasm about the value of metallography is inspiring and well founded. There is no doubt scientific metallurgy owes a great deal to the microscope.

The practical usefulness can likewise hardly be gainsaid, provided the operator has the indispensable advantage of a never-failing source of all kinds of old, worn-out or broken, and new material of various descriptions for everlasting comparison. Then the work with the microscope can be made really useful. Metallography is too recent a science to enable us as yet to say what its final status will be in the realm of applied metallurgy.

While there seems to be no analogy between glass and steel, yet Mr. Schumann happily furnishes us with the analogy of certain properties of these widely different substances by calling our attention to the necessity of the slow and gradual cooling of annealed glass, which necessity applies with equal force to steel of all grades. Slow cooling of annealed steel is one of the essentials of proper annealing.

The author feels himself honored to be able to remark on the discussion of his paper by such a past master of practical metallurgy, notably of cast iron, as is Mr. Outerbridge, Jr. How well Mr. Outerbridge has mastered the intricate properties of cast iron is shown by his pointing out that the releasing from its casting strains of an iron casting does not necessarily mean a strengthening of the cohesive force

which holds the particles together, nor a hardening of the individual particles, or crystals.

An unannealed car-wheel would not be safe, though the individual particles may be harder and more unyielding than those of an annealed wheel. But the percentage of elements in cast iron, other than pure iron, are considerable, and the changing of their form, due to the annealing, probably tends to loosen not only the cohesion between the particles, but, by deduction, also softens the latter.

This larger percentage of foreign elements in cast iron over cast steel is very likely also the cause of the fact that the size of an iron casting has a great deal to do with its final quality and suitableness for the purpose intended, while the absence of the largest percentage in cast steel of the foreign elements found in cast iron eliminates this marked difference between large and small castings, as found in cast iron.

Altogether, it appears to the author as if the discussion had emphasized the need for future study of the heat-treatment of steel, of the value of uniform methods of testing, of the use of the microscope, and of segregation.

ELECTRICAL SECTION.

Read at the stated meeting held November 28, 1899, and discussed at the stated meeting held February 27, 1900.

INCANDESCENT LAMPS.

By Francis W. Willcox.

(*Continued from p. 298.*)

THE WIDENED FIELD OF LAMP USE, ETC.

An entirely new and broad field was opened to lighting with the birth of electric light. Previously there was merely light—with electricity came illumination. Before the dynamo, light was used simply for necessity—hand in hand with the incandescent bulb came lighting for display, for ornament, for attractive effects.

The beautiful lighting display of the World's Fair, or more recently of the Omaha Exposition (the latter the most beautiful and best example of incandescent illumination the world has ever known), are a development and result made possible only with the electric light.

The adaptability of electric light and the special effects and results possible produced numbers of special applications which are well illustrated in lamps by the numerous special types now available. The first lamps were simple, plain multiple lighting lamps of about 13 candle-power. The first extension was into other candle-powers, such as the 32, the 50, the 100 and 150 and even 300 candle-power. Then the coloring and frosting of the glass gave a variety of effects. Next followed the miniature lamps. The first of these were battery lamps, made in 1883, for decorative lighting, at a Vanderbilt ball in New York. A large field was opened with this type. We now have the surgical lamp used to illuminate the interior of a person's anatomy, the dental lamp, the microscope lamp, the kinetoscope lamp, the bicycle lamp, the miner's lamp, the railway carriage lamp, and among the latest applications, the automobile lamp and the telephone lamp. This last is of special interest, and reveals the adaptability of the lamp to new purposes. To replace the sound or ear signal in telephone exchanges by the visual signal of a lighted lamp is a most original and valuable idea. These battery lamps are made in all shapes and sizes, pear-shaped, spherical and flat, from the size of the standard 16 candle down to the pea lamp, no larger than a fair-sized pea—in candle-power from $\frac{1}{4}$ of a candle to 24 candles, in voltage from 3 to 45 volts, and in economy from $2\frac{1}{2}$ watts per candle to 6 watts.

The unique character of miniature battery lamps for interior decorative effects prompted the production of miniature lamps that could be burned on the regular lighting circuits. These were produced to burn in series. These are what are called the series miniature lamps, and are most widely used for candelabra lighting, for electric signs and for decorative effects, for interior and out-of-doors, among

flowers and shrubbery, where a large number of units of light covering a large area are desired. The theatrical lamp, used for special stage effects and decorating dancers, is one of this class. They run in candle-power from 1 to 10 candle-power, and in various shapes, the twisted flame effect, the tubular, conical, bean-shaped and pear-shaped.

Series lamps, burning on regular lighting circuits, have of late years been a thorn in the side of the underwriters, and their rules and restrictions have resulted in the production of candelabra and miniature lamps adapted to burn in multiple on regular 100–125-volt circuits. This was quite an achievement in lamp manufacture, for many years considered impossible. Such lamps are in successful use, and to-day they have enlarged the field and application of miniature lamps and sign lighting.

A bank of incandescent lamps has always served as an excellent form of resistance. This has resulted in the production of the resistance lamp, a type largely used in connection with telegraph work. They are made of any desired resistance up to 1,000 ohms cold resistance for use with moderate currents, and are placed in tubular bulbs so as to minimize space. The desirability of having a lamp that could be turned down brought out the Edison night lamp, in which two filaments burn in series to give a dim light of about 1 candle. By the turn of a small thumbscrew, one of these filaments is cut out and the other burns at full 16 candle-power.

For stereopticon work the incandescent lamp is particularly well suited. A special lamp has been designed for this work, having a compact filament wound so as to make as near as possible a center or point of light. These are made standard at 50 candle-power, but higher candle-powers are obtainable.

For examining cask interiors, brewers and barrel cleaners require a long thin lamp that they may thrust through the bunghole and light up the interior of a cask. Hence the tubular or bunghole lamp, which is useful for confined or narrow places. It has recently been adapted with con-

siderable success for lighting the interior of counter showcases.

In decorative work, lighting show windows, etc., a short bulb is frequently desirable, and this has produced the round bulb type.

The buoys of New York Harbor are lighted by electricity, and for this work a special type of lamp was devised with permanent wire connections made through a watertight dummy base.

The United States navy requires a number of special types. All the regular Navy Cruiser lamps are 80 volts, and in special shaped bulbs. Then there is the navy signal lamp, a small lamp designed for quick lighting and extinguishing for signal work at night; also the torpedo lamp, instrument lamp and binnacle lamp. Unique and most interesting is the navy diving lamp, for use under water by divers. This is made of specially heavy thick glass, and protected from any damage from water or shock.

The European lamps are made in much smaller bulbs than are standard lamps in this country, so for the foreign trade a new lot of specialties are required.

The adoption of electricity for street railways has made a large demand for a type of lamp suitable for car lighting and running in series on the 500-volt railway circuit.

One of the latest and most interesting lamp specialties is the reflector lamp. This is made in a pumpkin-shaped bulb, of which the top side is covered with a silvered coating forming a perfect reflector. This makes a lamp of special value for a reading or desk lamp, or wherever the use of reflectors is required. The candle-power is concentrated in one direction to a threefold degree. Thus a normal 16 candle-power lamp in this special type will give about 50 candle-power in a downward direction.

Let us sum up and see how many varieties of lamps are required from the lamp manufacturer to-day.

In regular multiple lamps we have nine different candle-powers, and three different economies, and at least six different bases. Here are 162 types. In voltages we have

at least fifty regular types, which multiplies the number of types to 8,100.

These may be frosted, or any color—red, blue, green, amber or opal, which multiplies results by six, giving 48,600 types,	48,600
Now taking the Round Bulb we find varieties are	21,000
The tubular	21,000
Stereopticon	900
Resistance lamp (at least)	1,000
Multiple candelabra	500
Series candelabra	500
Special series (about)	500
Regular battery	40,000
Special battery (at least)	500
Navy lamps (about)	100
Foreign lamps (about)	10,000
Reflector lamp (about)	2,000
Night lamp (about)	400
Series lamps for alternating and arc circuit	100
Total	147,100

From the above we see the immense increase of types caused by the specializing of any one feature; for example, a special bulb, a special shape of filament, or a special efficiency. In regular multiple lighting lamps, we add a thousand or more types with each change to specials.

BETTER UNDERSTANDING OF LAMPS, ETC.

In early days the consideration for a lamp was simply that it should burn—the longer the better (so it was considered), irrespective of how much loss of light the lamp had suffered. No questions were asked as to the correctness or variation of candle-power or the limits of power consumed—in fact, the irregular voltage and absence of meters rendered it comparatively unimportant.

The first subject on which customers sought information was the power consumed by the lamp. A great amount of attention was directed to this feature, on account of the fact that dynamos were rated and sold by the number of lamps they could serve, and therefore a high economy lamp helped to increase the capacity of every dynamo. With

the introduction of the 3-watt lamp the question of mere initial efficiency became paramount. The real important feature, the amount of light given by a lamp, was, however, ignored or considered only in connection with its initial rating.

The manager of one of the large Edison stations actually stated, several years since, that he refused to believe his lamps ever changed in candle-power.

It is singular, when we consider it, how long it has taken lighting companies to understand the real essentials of a lamp. For years lamps were used as mere power-consuming devices—the light they gave was a secondary consideration. Some attention was called to the fact that lamps lost in candle-power while in service, but little heed was paid thereto. Many thought it a ruse on the manufacturer's part to dispose of more lamps, or if any considered subject at all seriously, they concluded it was a slight matter at most and decided to leave bad enough alone.

It has taken years to bring home the real considerations to electric lighting companies. These considerations are that a lamp is primarily designed and used to give light—that lighting companies are in the business of making and selling light, not power—and that the lamp is the chief essential of the lighting system.

Perhaps much of the blame for this lack of understanding can be laid to gas practice. Electric lighting has very naturally followed the principles and practice of the gas business. Gas fixtures and burners were supplied by the consumer, hence in electric lighting lamps should likewise be supplied, *i. e.*, purchased and renewed by the consumer. Gas tips were replaced only when broken, hence electric lamps should be used until they were burned out. The error of thus paralleling gas practice becomes evident when the difference in the conditions affecting each business is considered. In the gas business the quality and character of the light is dependent upon the quality of the gas—the old burner or tip (so long as it was not fouled) did not affect the result, and as these tips suffered little or no deterioration, there was no need of ever replacing them.

Mark the difference in the case of the electric light. Here the quality of the light (with voltage uniform) is not dependent upon the current—but upon the burner, *i. e.*, the lamp. Lamps upon the same circuit can be found that give markedly different results in light; moreover, the same lamp under same voltage will, when old, give but a fraction of the light it gave when new. These are well-attested facts, which clearly show that much in electric lighting depends upon the burner and in gas practically nothing. Hence, what might be good practice for gas is entirely wrong for electric lighting.

Recognition of these principles has caused the leading electric light companies to make the lamp a part of the apparatus owned and installed by the station. They have taken lamps out of the hands of the customer and supply all that are used on a free renewal basis. This prevents the customer from depreciating results and balking the efforts of the central station management to give a good service, as poor lighting is an inevitable result where the customer buys and renews his lamps.

This is one reason why electric lighting in the United States is so much in advance of the service abroad. In continental Europe the lamps are peddled out to customers of lighting companies by hardware stores, chandler shops and such. This not only renders a poor lighting service, but retards improvement in the art of lamp making. Under such conditions of supply and use of lamps, the lamp that is the cheapest and that will last longest is prized. This is almost invariably the lowest grade of lamp, and there exists, therefore, no incentive to the manufacturer to advance in his art. Relegating lamps to the hands of the customer always puts a ban upon quality. The control and supply of lamps by the lighting station on the contrary puts a premium upon quality. It is this excellent, wise and far-seeing policy on the part of the leading companies of the United States which has advanced their electric service and development beyond all other countries, and has aided the American manufacturer to produce an incandescent lamp that surpasses any in the world.

Nevertheless, knowledge of lamp essentials and correct use is not as widespread as it should be in the United States. There are hundreds of lighting companies in all parts of the country who are ignorant of or indifferent to a proper understanding of the subject. They will properly attend to the repair of boilers, engines or dynamos and all apparatus but that of the lamps, forgetting that the deterioration of the latter affects their service more noticeably and directly than any other cause. Enormous as is the consumption of lamps to-day, it is not half as large as it should be or would be were the proper and economical use of lamps observed by all lighting plants.

At the present low price of lamps, the cost of 600 hours to 1,000 hours of service is from eight to ten times the value of the lamp, and renders it economical, therefore, to the highest degree to use the highest efficiency possible and replace lamps frequently, since it is evident that the value of the current consumed by the lamp soon eats up the value of several lamps.

Byron says:

> "Better five hundred hours of glorious life
> Than a thousand dimly lit . . . "

and this is the ethics of lamp economy and service to-day.

Numbers of papers and discussions have treated on the subject of the lamp efficiency most economical for a given set of conditions and of the most economical life of lamps. All such discussions have their value, but the difficulty of applying formulæ to the varied and constantly changing conditions of different stations has limited their adoption in practice.

The present method is to follow a few simple rules formulated from the limiting conditions of practice.

The general rule as to efficiency of lamps is to use the highest practical efficiency suitable to the regulation. This gives the maximum station capacity in number of lamps and minimum cost of light to public.

As regards life, the leading practice is to limit the service of lamps to what is called their useful life. The useful life

is the period of time a lamp burns until it loses 20 per cent. of its candle-power—for example, the time required by a 16 candle-power lamp to drop to $12\frac{8}{10}$ candle-power.

This period is known for any given economy or make of lamp averaging to-day above 400 hours for the best 3·1-watt 100–125-volt lamps. The practical service method then is to renew lamps frequently enough to keep the average life within this limiting number of hours.

By periodical renewals of lamps on this basis the lighting service is kept as clean and bright as a room that is regularly swept and cleaned.

ADVANCE IN METHODS OF LAMP TESTING.

A correct understanding of lamps is obtainable chiefly through correct methods of test, and it is therefore of highest importance that the principles of correct testing should be thoroughly known and carefully observed. This is rendered still more important when we consider the numerous chances for error possible in the testing of lamps. There is hardly any practical testing work where persons with the best intentions but not thoroughly conversant with the subject are as apt to obtain discordant or misleading results.

It is gratifying to observe the growth of understanding on this subject, especially in the last two or three years.

Not more than eight years ago a paper was read before the Institute of Electrical Engineers giving an account of a lamp test as then conducted. It throws an interesting light on the lack of understanding thereon. In this test lamps of varied efficiencies running from $3\frac{1}{2}$ to $4\frac{1}{2}$ watts per candle were compared with one another in a life test without any allowance being made for this variation of efficiency. Further, results were considered chiefly as affecting the life of the lamps, and, although candle-power readings were taken, they were not reduced to percentages or any proper comparative basis.

This was on a footing with testing a lot of ropes of varying diameters by loading them all with the same weight and choosing those ropes as best that failed to break, irre-

spective of size of their diameter. In another instance a prominent professor of electrical engineering attempted to prove the value of so-called improvement in exhaustion by comparing 32 candle-power, 50-volt lamps thus exhausted with 16 candle-power, 100-volt lamps made in regular way. As a 32 candle-power lamp is naturally better than 16 candle-power, and likewise 50 volts better than 100, the error of this course becomes evident at once.

I desire to conclude this paper by a discussion of some of the features of lamp-testing, the latest methods of test, methods of determining and comparing the values of lamps and say a few words in regard to specifications and guarantees.

A lamp test can be said to be conclusive only when (1) the average product of each manufacturer is taken, and not specially selected lamps; (2) when a sufficient number of lamps are tested to secure a correct average result; (3) when the principles of correct testing are carefully observed, and (4) when the test is continued long enough to bring out the qualities of a lamp.

Modern methods of test cover three general features:

The physical examination for vacuum, defects in filament, and mechanical parts of the lamp.

The initial test for correctness of rating in candle-power and wattage at the rated voltage.

The life and candle-power performance test.

It is strange that the first two of these features of test were never formerly used. They are as necessary as the physical examination of the soldier to determine fitness for duty.

A lamp that cannot pass muster under physical requirements should never be accepted.

The effects of poor vacuum or of defective filaments are vital and far-reaching. Their elimination, therefore, becomes of the highest importance.

Poor vacuum results in the production of what are known as slumpers, *i. e.*, lamps that decline in candle-power rapidly and within a few hours—a most objectionable type. The test for vacuum is made with an induction coil drawing

about a ⅜-inch spark. By touching the base of the lamp to one of the secondary terminals of this coil a glow or fluorescence is caused inside the lamp. A well-exhausted lamp will either show no glow at all, or a slight flash or fluorescence.

Filaments are subject to a defect known as spots. This is a thin spot or a point of high resistance in the filament. By burning the lamp dull red these spots are seen very distinctly. This spot is a weakness in the carbon and will cause it to burn out. Spots, therefore, are very serious faults, as they cause early breakage and shorten the life of lamps. Tests for this defect can readily be made by burning the lamps two in series. The spots will show up as bright points against the dull red of the filament.

Another serious fault in filaments is discolored carbons. A good treated carbon has a uniformly bright, shiny steel gray surface over its entire length. A discolored carbon shows up a sooty black or brownish oily-looking surface, either partly or wholly covering the filament. Such discoloration causes a rapid loss of candle-power, a most undesirable result. They can be detected by an eye examination of the filament by light (filament not burning).

In some makes of lamps tested the percentage of physical faults has been found to be over 100 per cent. Numbers of barrels of different makes tested showed an average of fifty lamps to the barrel with discolored carbons. In another make thirty-two lamps with bad vacuum were found in one barrel.

The value of a physical inspection is unquestionable. It is also a ready means for the early detection of the incompetent or careless manufacturer and the rejection of poor lamps, thus saving time and trouble of any further test.

It should be an invariable rule in the purchase of lamps to reject any lot, samples from which are shown on test to contain 10 per cent. of any of these defects.

CORRECT INITIAL RATING OF CANDLE-POWER AND WATTAGE.

The value of close and uniform rating has only been appreciated in recent years. Of course it was not desired to

have lamps very low in candle-power, but no objection was made as long as the variation was not more than the eye could detect (about 3 or 4 candle-power). For lamps to run high in candle-power was by many considered as very desirable, and one manufacturer used to urge it as an advantage with his lamps that his 16 candle-power lamps gave more light than any other, and there was no doubt about it; some of his 16 candle-power lamps ran as high as 30 candle-power.

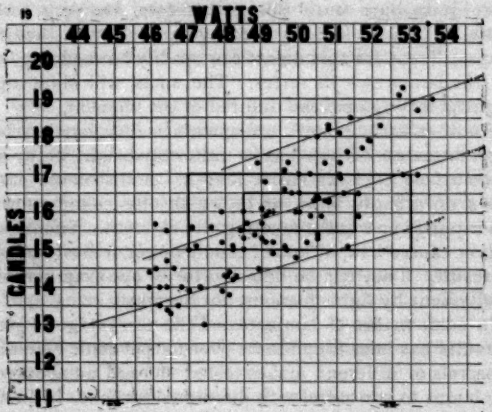


FIG. 2.—Target diagram showing initial measurements at marked voltage of 100 lamps, rated as 16 candle-power, 50-watt lamps.

For many years lamps were selected for watts, but the candle-power was allowed to wander over a considerable range. As the only testing instrument generally available was a wattmeter, skimping in candle-power passed unnoticed. This resulted in selling lamps for one efficiency which were really of a much lower efficiency, and there was no end of false conclusions.

With the introduction of the photometer, especially the portable photometer, most of this trouble ceased.

The importance of a close and uniform rating is very

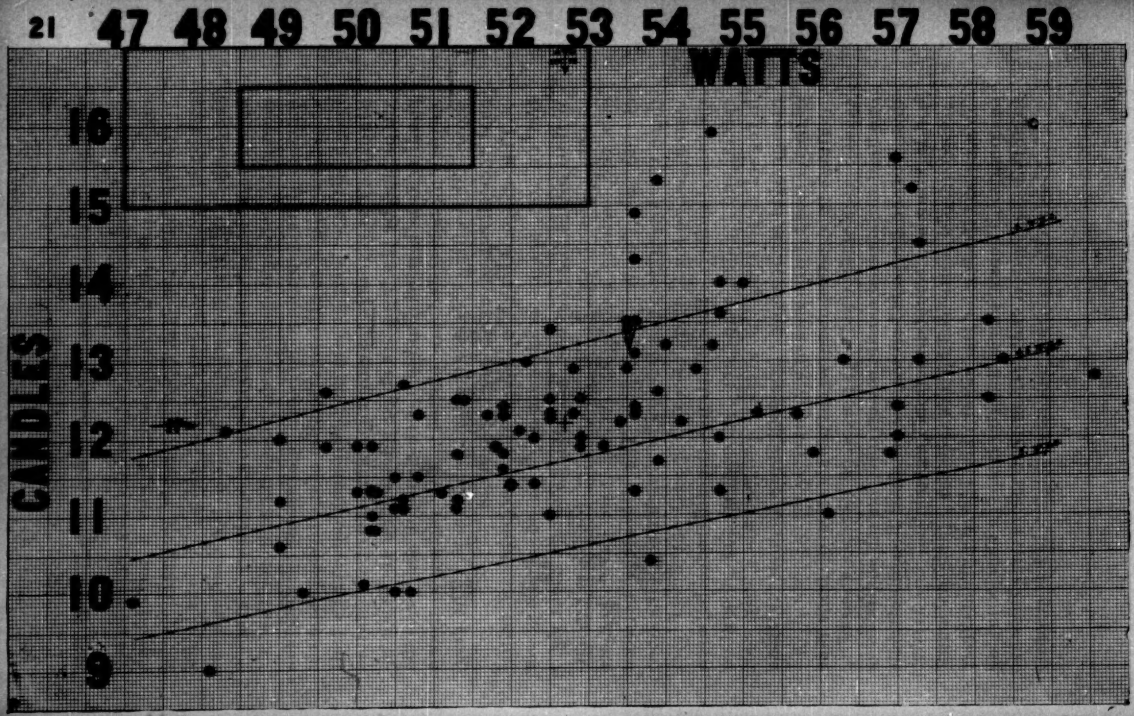


Fig. 1.—Curve sheet showing initial measurements at marked voltage of 103 lamps rated as 16 candle-power, 50-watt lamps, illustrating very

great. We can consider the value of it in a more interesting way by an examination of some results of initial tests.

A very clever way has been devised of diagraming the results of initial test. (See *Figs. 2, 3* and *4.*) This is called the target diagram method. By plotting the candle-power as ordinates and the watts as abscissæ, we have a graphic means of delineating accurate marksmanship (or the absence of it) in lamp manufacture. The center of diagram, or bull's eye, is made the intersection of the 16 candle-power line, and the 50-watt line (in case of 16 candle-power 3·1-watt lamps). The readings taken upon each lamp of a lot tested are plotted by placing a dot at the intersection of the corresponding candle-power and watts line, and in this way the entire number tested are plotted. The rectangle drawn shows the limiting specifications for good lamps. The diagonal lines shown give the watts per candle.

The diagram, *Fig. 2*, gives the result of an initial test on 100 lamps, which were sold for 16 candle-power 50-watt lamps at 116 volts. The lamps average about right in candle-power and watts, but scatter considerably outside the limiting rectangle. In *Fig. 3* we have 100 more of the same make of lamps. The difference between these two lots is that this last is a lot purchased in the open market, while the former lot was a special one direct from the manufacturer. This illustrates the difference between special and regular lamps, and the need of the precaution in regard to securing the average product for a test. An examination of diagram, *Fig. 3*, will serve to show the value of correct and uniform rating.

First—In candle-power we have a range of from nine candles to sixteen candles, an extreme difference equal to one-half of the rated candle-power. With such contrasts as this between lamps in service, the result will be, inevitably, bad lighting and complaints from customers.

Second—In watts we find a range from 47 to 60 watts. This means trouble for the meter man, and complaints from customers. There are over a dozen lamps in this lot taking from 15 to 20 per cent. more power than their rating calls for.

Third—In efficiency we note the lamps rated as 3·1-watt

lamps have a real efficiency of $4\frac{1}{2}$ watts. This shows the reduction in efficiency, resulting from low candle-power. Such lamps if measured for wattage would probably pass as 3·1-watt lamps, but when candle-power measurements are made the real watts per candle are shown to be $4\frac{1}{2}$. In a number of other cases of lamps incorrectly rated, lamps have been found to run high in candle-power, which is as grave a defect as running under candle-power. Lamps that run high in candle-power will be naturally strained at a higher efficiency than lamps nominally and correctly rated. Candle-power performance and service of these lamps will, therefore, be as erratic and non-uniform as is their initial rating. The heavy decline in candle-power due to incorrectly rated lamps

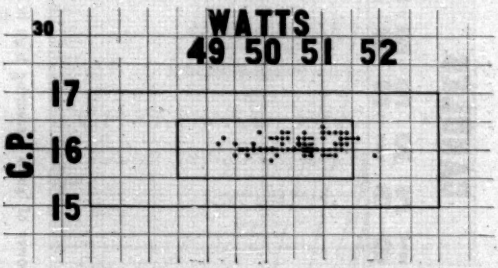


FIG. 4.—Target diagram showing results of correctly rated lamps.

(running high in candle-power) is well shown in diagram, *Fig. 5*, which gives performance of a number of lamps (rated as 16 candle-power) giving maximum candle-power readings.

Thus in a threefold degree is close and uniform rating necessary if we desire to secure good life and the best service results. It takes experience and ability to make lamps correct and uniform, and it costs money to select them to their close limits. Any attempt here to save money by the manufacturer would add greatly to the cost of renewals to the purchaser. It is, therefore, money saved for the purchaser to pay for these essentials, that is, purchase from the competent and capable manufacturer.

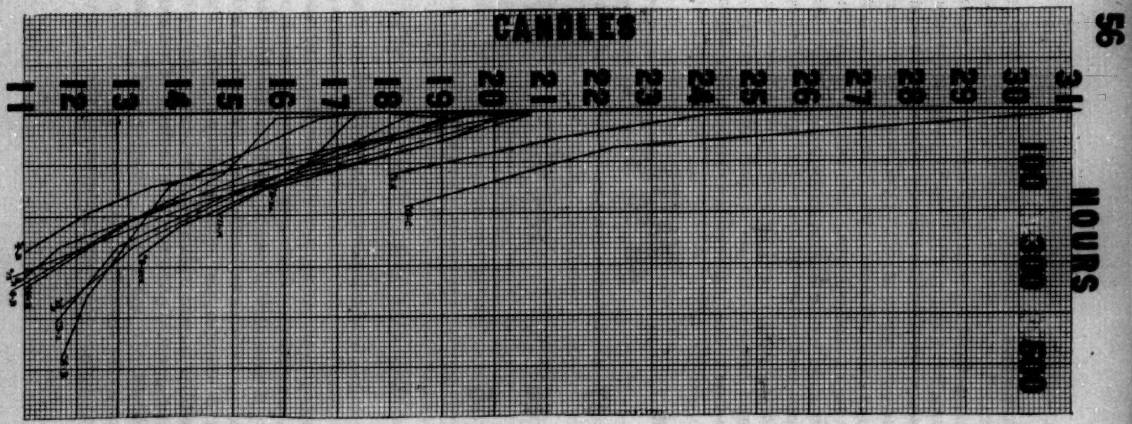


FIG. 5.—Showing rapid decline in candle-power of lamps measuring high in candle-power—well above their nominal rating.

Diagram, *Fig. 4*, shows the results of test on 100 lamps of a high-grade lamp, and illustrates what may be expected of well-made lamps.

It is to the interest of all purchasers to require lamps to conform to the one candle, 3-watt limits referred to above, by testing samples from each lot purchased, and rejecting every lot failing of these requirements.

There is no need of farther testing any lamps that fail in this respect.

In comparative tests of lamps, as many lamps as possible should be tested for initial rating—from 50 to 100 or more. This gives a full average of results, and besides provides a sufficient quantity from which to choose lamps of the efficiency taken as a basis for the candle-power performance test.

[*To be concluded.*]

CHEMICAL SECTION.

Stated Meeting, held January 16, 1900.

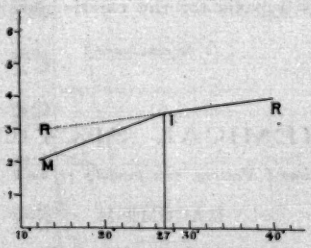
RACEMISM.

By Robert Hart Bradbury.

(*Concluded from page 314.*)

Summing up, then, the equilibrium between the two antimeres, the racemic compound and their saturated solution is very much the same as that between two single salts, their double salt and the solution, and the phase rule applies to it in all its fruitful simplicity. The inversion point is the point at which the liquid in equilibrium with the racemic form as solid phase has the same composition as that in equilibrium with the mixture of the antimeres, *i. e.*, it is the temperature at which the solubilities are the same. Or it is the temperature at which the solubility curves for the mixture and for the compound intersect.

The diagram, borrowed from Van't Hoff,* represents the case of sodium ammonium racemate. The abscissas are temperatures and the ordinates concentrations expressed in molecular weights in 100 molecular weights of water. MI is the solubility curve of the mixture of the single tartrates. So far as this curve is concerned, the quantity of the tartrates in the solid mixture is a matter of indifference. The only condition is that they must both be present as solid phases. RI is the solubility curve of the pure racemic compound below 27°. Since the solubility of the racemate is greater than that of the mixture, it is clear that up to 27° the solution of the former is supersaturated with respect to



Solubility curves of sodium ammonium racemate and of the mixture of the two tartrates.

the latter. In this part of the curve the addition of the solid mixture will cause the crystallization of both antimeres from the solution of the racemate. I is the inversion point. Here the two curves intersect and here both solutions contain about 3·5 molecular weights of each antimere, the temperature being 27°. If we imagine the curve MI prolonged through I, it will evidently be above RI and be unstable with respect to it in exactly the same way as RI is unstable with respect to MI below the inversion point, that is, the addition of the solid racemate to the saturated solution of the mixture will produce crystallization of the

* "Bildung und Spaltung von Doppelsalzen," p. 83 (1897).

racemate, until the point corresponding to that temperature on the curve RI is reached. Thus the reversal of stability which occurs at the inversion point is entirely a matter of solubility and the whole phenomenon will be altered by the use of some other liquid in which the solubilities of the enantiomorphs and the racemic form are different.

The curve IR ends to the right in another transformation which takes place at about 36°. This is the conversion of the sodium ammonium racemate into the single sodium and ammonium racemates:

$$2(NaNH_4C_4H_4O_6)_2\,2H_2O = (Na_2C_4H_4O_6)_2 + [(NH_4)_2C_4H_4O_6]_2 + 4H_2O.$$

Both the single salts are still racemic, and this change does not concern us at present. But it is interesting to notice that the existence of sodium ammonium racemate in stable equilibrium in contact with water is limited by temperature in two directions. It is only possible between 27° and 36°.

We must now consider the phenomenon of partial racemism (Halbracemie). The name is due to Emil Fischer.* In the course of his marvellous work on the sugars he was naturally brought continually into contact with the phenomena of optical isomerism. From the fundamental stereo-chemical idea it is clear that the presence of four asymmetric carbon atoms in the aldohexoses and the acids derived from them will bring about the existence of sixteen optically active isomerides, grouped in pairs. The members of each pair will possess the antimeric relation, that is, each isomer will combine with one other—its antimere—to a racemic compound. There will be eight racemic compounds. Of course the members of different pairs are remarkably similar in composition, the difference between them being not structural, but geometrical only. The interesting possibility occurred to Fischer that substances not mirror images might combine after the fashion of antimeres to compounds which, while they would be closely

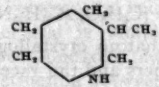
* *Berichte*, xxvii, 3225 (1894).

analogous to the true racemic forms, still might be expected to exhibit differences from them. Two of the pairs above referred to are d- and l-mannonic acid, antimeres, and d- and l-gluconic acid, also antimeres. Fischer attempted to obtain compounds of d-gluconic acid with d- and l-mannonic acid, but without success. He concluded from this that there is no special inclination to the formation of partially racemic compounds, and that the realization of the conception is improbable. We owe the complete certainty that such compounds exist to Ladenburg, who was led in this direction by his synthesis of conine. His work in connection with racemism has been marred by several important blunders, and it is pleasant to be able to credit him in this matter with completely making out his point.

The salts of the two tartaric acids exhibit the phenomena of racemism in the same way as the acids themselves. The replacement of the hydrogen by a metal or by a symmetrical inactive radicle leaves the enantiomorphous relation of the molecules untouched and the behavior associated with this relation persists. But when the hydrogen is replaced by an unsymmetrical active radicle everything is completely altered. The molecules of the two salts are no longer mirror images, the enantiomorphous relation has disappeared and with it the whole mass of correlated phenomena. Thus the salts of the two tartaric acids with the same active base are no longer antimeres and their densities, solubilities, melting points, content of water of crystallization are different, while nothing can be said *a priori* with respect to the crystalline form, except that in all probability the two will crystallize differently.

Now the essence of what is known at present about partial racemism is that compounds like these, not antimeres, can unite, after the manner of enantiomorphs, to form compounds which are called partially racemic (halbracemische Verbindungen). It is easy, reasoning in a similar way, to arrive at the conclusion that the two salts of the two antimeres of an optically active base (for instance, the salts of d- and l-conine) with the same optically active acid

(for example, d-tartaric acid) will exhibit exactly similar behavior. It is possible that the two salts which the base β pipecoline



forms with d-tartaric acid are an instance of this. The base contains an asymmetric carbon atom and accordingly a d- and an l-form are known. It is very possible that the acid d-tartrates of the antimeres unite to a partially racemic compound, but the evidence is not conclusive.* Another instance is the d-tartrate of racemic tetrahydropapaverin.† It is important to notice that partially racemic compounds in general will be optically active, since, from the nature of the case, the activity of either the acid or the base is uncompensated. The optical activity of r-tetrahydropapaverin d-tartrate, for instance, is of the same order as that of the metallic tartrates. Another partially racemic compound is the salt which r.pyrotartaric acid $CH_3CH(CO_2H)CH_2CO_2H$ forms with quinine. Here the acid is responsible for the phenomenon.‡ By far the most completely investigated case is that of the neutral tartrates of strychnine.§ Here the difference between partial and complete racemism is well brought out. We have seen that the properties of antimeres, except the opposed rotation and, when it is present, the enantiomorphous crystallization, are rigorously identical. But the d-tartrate of strychnine contains $6\frac{1}{2}$ and the l-tartrate $3\frac{1}{2}$ of water of crystallization. The melting point of the former is 228°, that of the latter 242°. And the l-tartrate possesses a much higher density and is much less soluble in water than the d-tartrate. When a mixture of equimolecular quantities of the two salts is moistened with about two molecular

* Ladenburg, *Berichte*, xxvii, 75.
† Pope and Peachey, *Zeitsch. für Krystallographie*, xxxi, 11.
‡ Ladenburg, *Berichte*, xxxi, 524.
§ Ladenburg, *Berichte*, xxxi, 1969.

weights of water the mass becomes dry, owing to the following reaction:

$$d\text{-}C_4H_6O_6.2C_{21}H_{22}N_2O_2.7H_2O + l\text{-}C_4H_6O_6.2C_{21}H_{22}N_2O_2.3\tfrac{1}{2}H_2O + 2\tfrac{1}{2}H_2O = (C_4H_6O_6.2C_{21}H_{22}N_2O_2)_2.13H_2O.$$

It is unfortunate that no crystallographic investigation was made of the isomers and the racemic compound. The results would have been extremely interesting. Of course, there must be here, as in the case of racemic compounds, one place on the thermometric scale where the three solid phases are in equilibrium with solution and vapor. This occurs at 29·5°.* The chief difference between the two cases is that, owing to the unequal solubility of the isomers, the symmetry of the solution curves will disappear and the liquid phase will not, in general, contain equal quantities of the isomers, either at the inversion point or at other temperatures. This point must be insisted upon because Ladenburg found nearly equal quantities of the d- and l-strychnine tartrates in the solution at 30°,† and uses this fact to confirm his contention that, in general, in the case of partially racemic compounds, the solution at the inversion point will contain equal quantities of the two salts. This conclusion is quite erroneous.‡

Leaving the subject of partial racemism we return to true antimeric compounds. The closer the chemical similarity of two substances, the more likely they are to produce solid solutions, or mix-crystals with each other. The fact, therefore, that antimeres do this is not surprising. Our knowledge of this matter has been attained very recently, and is due entirely to the fascinating researches of Kipping and Pope.§ When the antimeres grow together to a solid solution the result is what Kipping and Pope have named the pseudo-racemic mix-crystal. This lies between the mere mechanical mixture and the chemical combination of the two, the racemic compound. All three

* Ladenburg, *Berichte*, xxxii, 50 (1899).
† Ladenburg, *Berichte*, xxxii, 52.
‡ *Cf.* Bakhuis Roozeboom, *Zeit. f. Physikal. Chem.*, xxviii, 503.
§ *Zeit. f. Krystallographie*, xxx, 443.

are inactive by external compensation and must not be confused with the meso-form, *e. g.*, mesotartaric acid, in which one-half of the molecule reverses the action of the other half. The distinction between the inactive mixture and the pseudo-racemic mix-crystal is simple. The latter is a homogeneous substance—one solid phase, the former a mixture of two. The distinction between the mix-crystal and the racemic compound is a special case of the principle that a compound is a form of matter having its own properties, while the properties of a solid solution* are those of the two constituents, those of each affected, to some extent, by the presence of the other. Thus the crystalline form, density, solubility and so on of a racemic compound will, in general, be quite different from those of the antimeres. But the crystalline form of the pseudo-racemic mix-crystal is so similar to that of the antimeres that crystallographic distinction is difficult, though undoubtedly the angles are slightly different. The faces show multiple reflections and the other characters that crystallographists associate with impure crystals. The density will be close to that of the antimeres, but slight differences will occur. Kipping and Pope consider that the mix-crystal will always melt at the same temperature as the antimeres, and that the inactive mixture will also have the same melting point. Our knowledge of the melting points of isomorphous mixtures is regrettably incomplete,† but it is very unlikely that they are right in this. It is improbable that the statement applies even to the mix-crystal, while, so far as the inactive mixture is concerned, it is certainly incorrect. The only mix-crystal which could well be confused with the racemic compound, that is, the inactive one, containing 50 per cent. of each antimere, melts like a chemical compound at a definite temperature, and does not display the progressive melting of

* For the purposes of the present paper it is unnecessary to distinguish between mix-crystals and solid solutions in general.

† *Cf.* Küster, *Zeit. für Physikal. Chem.*, viii, 577 (1891).
Centnerszwer, *Zeit. für Physikal. Chem.*, xxix, 715 (1899).
Roozeboom, *Zeit. für Physikal. Chem.*, xxviii, 510 (1899).
Roozeboom, *Zeit. für Physikal. Chem.*, xxx, 385 (1899).

solid solutions. Nothing can be said, *a priori*, about the location of the melting point with reference to that of the antimeres. It may be higher, lower, or at the same temperature.

THE RECOGNITION OF THE PHENOMENON.

The question of the recognition of racemism, of the methods of distinguishing a racemic compound from an inactive mixture or a mix-crystal is of great importance to organic chemistry, and has of late received much attention. Emil Fischer was one of the first to turn his attention to the matter.* He points out that it is only in the solid state that the phenomenon is known with certainty, and remarks that indications as to whether an inactive liquid is racemic or a mere mixture may, perhaps, be obtained from alterations in density, refractive power, magnetic rotatory power, or evolution or absorption of heat when the antimeres are mixed. This was only a tentative statement, and it may be remarked that two of the criteria are of doubtful value, since evolution of heat and alteration of density are common in the mixing of liquids when there is no reason to assume the occurrence of chemical combination. Fischer himself later† called attention to this in connection with the racemism of inactive synthetic conine. Ladenburg had found that when d- and l- conine are mixed in equal quantities there is a slight fall of temperature, and had concluded from this that the liquid is a racemic compound. This, however, proves nothing. Among common liquids, which produce considerable temperature changes when mixed, are alcohol, ether, carbon disulphide and chloroform. Ladenburg‡ has shown that when ether and chloroform are mixed at ordinary temperatures in the right quantities the temperature rises 12°.

In this case there is considerable contraction, while in the mixing of d- and l-conines the density remains unchanged. Ladenburg lays stress on this fact and contends that evolution or absorption of heat without simul-

* *Berichte*, xxvii, 3225.
† *Berichte*, xxviii, 1153.
‡ *Berichte*, xxviii, 1993.

taneous change of density is good evidence of chemical combination, and, therefore, of the racemism of the liquid. It seems to me that the value of this criterion is very slight, for the condition of no alteration in density would almost never be fulfilled, and, when it was, it itself would offer stronger evidence against the combination than a small temperature change would give in its favor. Küster* makes the interesting point that the behavior of the temperature change in time in this case indicates chemical combination. Ordinary temperature changes taking place during the mixture of liquids are instantaneous, while when d- and l-conine are mixed the lowering of temperature requires half an hour to become complete. This indicates a slow reaction going on in the mixed liquid and a correspondingly gradual absorption of heat,

$$\text{d-conine} + \text{l-conine} = \text{r-conine}.$$

The literature of liquid racemic compounds is small in amount and inconclusive in character. New methods of attacking the problem are needed.†

With respect to the existence of racemic compounds in solution, the same remarks may be made. From the lack of evolution or absorption of heat when solutions of the antimeres are mixed,‡ and from the conductivity and cryoscopic behavior of solutions of racemic compounds the conclusion has been reached that only the antimeres exist in the liquid, but it is improbable that this is correct. In some cases the solubility relations render it very probable that a considerable proportion of the dissolved substance is racemic.§

It must be remembered that a number of molecular species must be taken into account in discussing the state of things in a liquid in equilibrium with a racemic compound as solid phase. Since most racemic compounds are electrolytes—acids, bases or salts—there may be present the ions of both antimeres as well as undissociated molecules of both and

* *Berichte*, xxxi, 1847.
† *Cf.* Pope and Peachey, *Proc. Chem. Soc.*, xv, 201 (1899).
‡ Jahn, "Wied. Annalen," xliii, 306 (1891).
§ Küster, *Berichte*, xxxi, 1847 (1898).

polymeric molecules. Then the racemic compound may be present in the same three conditions, and the fact that its molecular weight is unknown must not be lost sight of. If the racemic compound happens to be a salt of a weak acid or base the possibility of hydrolysis further complicates matters. It will be seen that the problem is not an easy one. It becomes simpler in the case of a compound not an electrolyte where electrolytic dissociation and hydrolysis are impossible. Such compounds are not difficult to find—among the sugars, for instance—and the study of their condition in solution would, it seems to me, afford very interesting results. With respect to the possibility of racemism in gases our ignorance is complete. Most racemic compounds cannot be obtained in this condition at all, and the only substances which have been investigated are the esters of racemic acids, *e. g.*, di-methyl racemate. The result of density determinations is always a molecular weight corresponding to that of one antimere, so that it seems that the racemic compound dissociates into the antimeres on being vaporized.

As Fischer has acutely remarked, it is only in the solid state that the characteristic signs of the racemic condition are apparent. We are only concerned with the differentiation of the racemic compound from the other inactive forms, the inactive mixture containing equal quantities of each enantiomorph and the pseudo-racemic mix-crystal of the same composition. The surest way of making the distinction is crystallographic investigation. The conglomerate will often be a mixture of two different crystalline forms bearing enantiomorphous relations to each other. The racemic compound will consist of homogeneous crystals of one species and its form will be absolutely different from that of the antimeres, whether the latter are enantiomorphs or not. The pseudo-racemic mix-crystal will exhibit multiple reflections and the other imperfections which are associated with crystals of impure substances. In form it will possess an extraordinary similarity to the antimeres, but there will be small differences in the angles. Its inactivity at once distinguishes it from the enantiomorphs and its crystalline form

from the racemic compound. Unfortunately the method requires good crystals and is not always applicable.

Fischer has pointed out that at times the distinction can be made by chemical analysis, for example, when the antimeres are salts containing water and the inactive substance contains either more or less water. Such cases have long been known among the tartrates. Inactive calcium mannonate is anhydrous, while the antimeres contain water of crystallization. Kipping and Pope* consider that all mixtures of the antimeres and all solid solutions of them will melt at the same temperature as the antimeres separately. Hence, when the inactive substance has a different melting point it is to be regarded as a racemic compound. This conclusion is certainly erroneous. Fischer's cautious statement that when the inactive substance has a higher melting point than the antimeres it is certainly racemic, while, if lower, no decision can be made,† has won the general assent of chemists, but even this generalization must be qualified. The possession of a higher melting point than that of the antimeres does, indeed, distinguish the racemic compound from the inactive mixture, but not from the pseudo-racemic mix-crystal, since this may melt either higher or lower than the antimeres separately. The inactive mixture of equal quantities of the antimeres has the lowest melting point of all conglomerates of the two, while the pseudo-racemic mix-crystal has either the lowest melting point of all solid solutions, or else the highest, unless the melting point is the same for all concentrations. Finally, the racemic compound is simply a pure substance with its own melting point. The effect of mixing a little of either antimere to the racemic compound will be to lower the melting point just as any other foreign substance would do. But the effect on the inactive conglomerate will be to raise the melting point. With the pseudo-racemic mix-crystal the same addition may raise the melting point or lower it, or leave it unchanged.

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* *Zeitschrift für Krystallographie*, xxx, 443; also *Jour. Chem. Soc.*, lxxi, 989.

† *Berichte*, xxvii, 3225.

It is clear that we can easily distinguish the compound from the conglomerate by means of melting point determinations, but not from the mix-crystal. This same distinction can be very readily made by a solubility determination. Consider a solvent in equilibrium with both enantiomorphs as solid phases. Since both dissolve in equal quantity at all temperatures the liquid will be inactive. Now, let either solid antimere be added to it. The solution being already saturated with respect to this substance, the sole effect is to increase the amount of that particular solid phase; the liquid remains inactive. But with the liquid in equilibrium with the solid racemic compound everything is different. None of the antimeres being present as solid phase, the solution is unsaturated with respect to it, and it dissolves at once, the liquid of course becoming active. It follows from the fundamental principles of the phase rule that the liquid in equilibrium with a pseudo-racemic mix-crystal will become active on adding either antimere. So, also, when the inactive substance is a liquid the solution over it will be made active by adding either antimere, whether the liquid is racemic or not. The criterion is, therefore, completely inapplicable to liquids, and Ladenburg's recent attempt to prove the racemic character of inactive conine and limonene by means of it is a surprising blunder.

I have given here only those facts with respect to melting point and solubility which are most likely to be of value to the organic chemist. The exhaustive discussion of the solubility and fusion relations of mixtures and compounds of enantiomorphous isomers is inseparably bound up with the general doctrine of the phase rule, and to attempt anything like a detailed treatment of the subject within the limits of the present paper is impossible.*

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* Bakhuis Roozeboom has recently published two papers on this subject. The shorter one (*Berichte*, xxxii, 537) is an abridgment of the other, and, like all abridgments, very unsatisfactory reading, but the complete paper (*Zeit. Physikal. Chem.*, xxviii, 494) will become a classic in chemical literature. It is remarkable for exactness and for lucidity of statement.

THE SEPARATION OF THE ANTIMERES.

Owing to the identical solubilities of the antimeres and to the sameness of their chemical behavior toward symmetrical substances, their separation by ordinary methods is impossible. For the same reason when a carbon atom becomes asymmetric the product is the inactive mixture or the racemic compound, the two isomers being produced in equal quantities. This impossibility of producing one enantiomorph without the other from inactive substances and under symmetrical non-selective working conditions was a favorite thesis of Pasteur, and a recent revival of the idea in a somewhat different form* has given rise to much controversy. The difference between the two views is that Pasteur's was simply a tentative working hypothesis which he was ready to abandon at any time, while Japp regards the production of a single antimere under the conditions stated as inconceivable and the impossibility of doing so as a permanent limit to the achievements of our science.

The conclusion is that the optically active substances which exist in nature must have been produced by the action of some unsymmetrical force which appeared upon the scene at the same time as life, and which, presumably, is identical with the vital force of the older biologists. Pasteur's statement was valuable in his hands because it often led to predictions which were verified by experiment, but it is clear that Japp's extension of it does not fulfil the requirements of a scientific hypothesis.

Most of Japp's critics were not chemists, and were hampered by lack of acquaintance with the subject under discussion, but two of them, whom we can hardly accuse of unfamiliarity with chemical matters, have shown that it is perfectly possible to imagine symmetrical conditions under which one enantiomorph would be produced to the exclusion of the other.† ‡

Pasteur worked out three methods of separating the

* Japp, *Nature*, lviii, 452, and many following numbers.
† Ostwald, *Zeit. Physikal. Chem.*, xxix, 347 (*Ref.*)
‡ P. Frankland, *Nature*, lix, 30.

antimeres. The first is applicable only to a mixture of well-developed enantiomorphous crystals, and consists in the simple picking out of the two kinds. Thus when the solution of sodium ammonium racemate crystallizes, right and left enantiomorphous forms are deposited side by side. Each can be made to yield its own kind of acid, and so the preparation of a whole series of antimeres becomes possible. The method has not been found generally applicable, because it is difficult or impossible in most cases to obtain crystals distinct enough for the selection to be made.

The second method applies only when the enantiomorphous substances to be separated are bases or acids, a very frequent case.

If they are acids the mixture is treated with an active base; if bases, with an active acid. In either case two salts will be produced in the solution and one of them will crystallize first, and by recrystallization can be obtained pure. Thus Pasteur separated d- and l-tartaric acids by adding cinchonine when the cinchonine l-tartrate crystallized out. He considered that l-tartaric acid had a stronger affinity for the base than the dextro-acid, and hence its salt was formed, the latter remaining free. This view is still general, but since it is in complete discord with the doctrine of affinity and with the results of experiment it must be abandoned.

Antimeric acids, being equally dissociated at all dilutions, must exhibit the same affinity toward all bases, asymmetric or not, and the same thing is true of antimeric bases. This statement has been abundantly confirmed by direct experiment.* † Thus the heats of neutralization of the two tartaric acids with the active bases, morphine and nicotine, are the same. The dextro- and lævo-tartrate of cinchonine raise the boiling point of water to an equal extent, demonstrating that the electrolytic dissociation and hydrolysis of the salts at equivalent dilutions are identical. This could

* Jahn, "Wiedemann's Annalen," xliii, 306 (1891).
† Marckwald and Chwolles, *Berichte*, xxxi, 783 (1898).

not be the case if the affinities of the two acids for the base were different. Finally, when inactive methyl-ethyl-acetic acid or inactive mandelic acid is one-half neutralized with brucine in aqueous solution the rest of the acid, extracted by ether from the liquid, is inactive. Clearly, if the brucine had combined preferably with either acid the other one would have predominated in the ether extract.

The fact is that the crystallization of one salt in such separations is purely an affair of solubility. Both salts are produced in equal quantities, but they are no longer antimeres and differ in all respects, solubility included. That one crystallizes first which is least soluble under the conditions of the experiment. This method of separation has been largely employed by Ladenburg and others. For the separation of asymmetric bases, d-tartaric acid is often used; for that of enantiomorphous acids, quinine, cinchonine and strychnine are in favor. Pasteur's third method was the biological one. The solution of the inactive substance is sown with the spores of a mould, penicillium is frequently used, and nutrient material is added. One isomer is destroyed, the other remains. The method has been successfully employed, though the destruction of one isomer is a disadvantage, especially as the choice rests with the mould and not with the investigator. Sometimes the substance acts as a poison for the micro-organism; it does not grow, and the product is unchanged in quantity and still inactive. This happened, for instance, when Ladenburg attempted to separate synthetic conine by this method. Recently two new methods of separation have been added to these classical ones. Kipping and Pope have found that the solubilities of the antimeres are no longer identical when the solution contains a third asymmetric substance. This substance must have no chemical action upon the antimeres or the dissolving liquid. Thus, when inactive d- l-sodium ammonium tartrate was crystallized from water containing dextrose, the first crop of crystals contained a considerable excess of dextro-tartrate. I consider it probable that the solubility of the antimeres will be found to be different where the solvent itself is active. Suitable solvents could

be found, for instance, among the active alcohols. Further, it should not be difficult to find a case where the solvent crystallizes with the antimeres as alcohol of crystallization. The question whether each would separate with the same number of molecules of the solvent as they always do with water and symmetrical solvents in general is of great interest.

The second of the new methods rests upon the fact that the speeds with which the antimeres react with an asymmetric substance are different.* If, then, the process is interrupted before the reaction is complete, the unchanged substance will contain an excess of one antimere and the reaction product of another, and from either, by suitable methods, the isomer which predominates can be isolated. For instance, when lævo-menthol is heated with inactive mandelic acid the dextro-acid is estrified most rapidly, and if the reaction is interrupted after it has continued for an hour, lævo-mandelic acid can be isolated unchanged from the product. Further, the esters of the two mandelic acids with lævo-menthol, and in general the esters of antimeric acids with an asymmetric alcohol, are no longer enantiomorphs and exhibit chemical differences on which a separation can be based. When an equimolecular mixture of lævo-menthyl lævo-mandelate and lævo-menthyl dextro-mandelate is saponified, the dextro-ester is attacked more rapidly, and if the saponification is carried out in two fractions the acid from the first is dextro-rotatory, that from the second lævo-rotatory. Of course, both acids are mixtures, but from both that isomer which is present in excess can be isolated. The method of Marckwald and McKenzie is new, and owing to the extraordinary rapidity with which work accumulates in this field, the authors have felt constrained to publish it with only a few preliminary experiments. Many difficulties are to be overcome before the procedure can rank as a useful method of separating enantiomorphs. The authors have promised to proceed imme-

* Marckwald and McKenzie, *Berichte*, xxxii, 2130 (1899).

diately with the experimental investigation, and chemists will await their results with much interest.

CENTRAL MANUAL TRAINING SCHOOL,
DEPARTMENT OF CHEMISTRY,
PHILADELPHIA, January 16, 1900.

ADDENDUM.—In the first part of this paper attention is called to the fact that the phenomena of racemism are dependent upon the presence in the molecule of a carbon atom united with four different radicles, or of a nitrogen atom united with five different groups, an asymmetric carbon or an asymmetric nitrogen atom. Since this was written Pope and Peachey,* in a most interesting preliminary communication, have announced the preparation of racemic derivatives of tetravalent tin, and the asymmetric tin atom has been added to our science.

The authors started from the tin trimethyl iodide of Ladenburg and Cahours. From this they obtained methyl-ethyl-n-propyl tin iodide by methods which are indicated in the following equations:

$$2Sn(CH_3)_3I + Zn(C_2H_5)_2 = 2Sn(CH_3)_3C_2H_5 + ZnI_2,$$
$$Sn(CH_3)_3C_2H_5 + I_2 = Sn(CH_3)_2C_2H_5I + CH_3I,$$
$$2Sn(CH_3)_2(C_2H_5)I + Zn(C_3H_7)_2 = 2Sn(CH_3)_2(C_2H_5)C_3H_7 + ZnI_2,$$
$$Sn(CH_3)_2(C_2H_5)(C_3H_7) + I_2 = Sn(CH_3)(C_2H_5)(C_3H_7)I + CH_3I.$$

This methyl-ethyl-n-propyl tin iodide contains an asymmetric tin atom. It is a yellow oil, almost insoluble in water. Its vapor attacks the mucous membranes. Like all the products of synthesis, it is inactive by external compensation, equal quantities of the d- and l-isomers being produced.

In order to obtain an active product the substance was treated with the silver salt of d-camphor-sulphonic acid in aqueous solution, when d-methyl-ethyl-n-propyl tin iodide d-camphor sulphonate,

$$Sn(CH_3)(C_2H_5)(C_3H_7)(C_{10}H_{15}OSO_3),$$

separates. This is strongly dextro-rotatory. The corresponding lævo-salt could not be obtained. When the

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**Proceed. Chem. Soc.*, xvi, 42 (1900).

mother liquor was evaporated nothing but fresh quantities of the dextro-salt were obtained, an extremely interesting phenomenon and one readily explained by rapid auto-racemization restoring the equilibrium continually disturbed by the separation of the less soluble dextro-salt. Finally, when the solution of this salt is treated with aqueous KI, d-methyl-ethyl-n-propyl tin iodide separates as a yellow oil which was decidedly dextro-rotatory. The mobility of the radicles attached to quadrivalent tin appears to be surprisingly great, and at times the oil is inactive, in consequence of auto-racemization. Walden has shown that the same thing takes place with asymmetric carbon compounds, but very much more slowly.

The authors are of the opinion that all compounds with enantiomorphous molecular configuration, at least all those in which the asymmetric atom belongs to Mendelejeff's fourth or fifth group, will be found to exhibit optical activity. They are engaged in the attempt to prepare active derivatives of tetravalent lead.

PHOTOGRAPHIC AND MICROSCOPIC BRANCH.

Stated meeting held Tuesday, December 5, 1899.

IMPROVED METHOD FOR MOUNTING PRINTS.

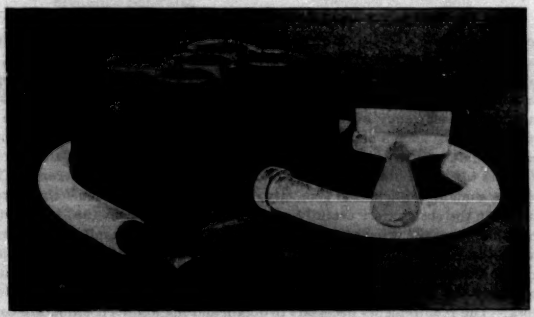
By John G. Baker,
Member of the Section.

Often, the troublesome part to the amateur photographer is the mounting of the print. The cause of this is that, by the old method, it requires some practice, which he has not had; for the number of prints he has to mount is so small that he prefers to get them in shape for the album, especially after he has tried his hand at mounting the first few prints in the old way and making a mess of it.

Now, to remedy this trouble I have contrived the device I have here this evening and of which I will give you a brief description and a practical demonstration.

This invention is for the mounting of squeezed or other dry prints. The method has several advantages. It is very simple, neat and clean; stops all daubing of the prints and leaves them and the hands quite clean. The mounts have less tendency to curl, and as the prints are quite stiff, they are more readily handled, and if they are to be colored, it may be done before they are mounted, and there is, therefore, less danger of soiling the mount. To the inexperienced person this method is well adapted, since it requires but very little skill to make quite a neat job of it.

The arrangement consists first of a block of wood, which is called the squeezed print mounter. This block is made



about $\frac{1}{8}$ inch smaller than the print to be mounted, and its upper surface is formed with a series of circular grooves, each of which is in communication with its neighbor, and the whole system in communication with the outlet pipe near the bottom of the mounter. To this outlet is attached a piece of rubber hose, and to facilitate the handling of the prints a small paddle is also added. To keep the prints more firmly in place, the upper surface of the mounter is coated with a solution of rubber cement, made quite thin with benzine. This coating must be renewed whenever the surface gets too smooth to hold the prints firmly.

To use this mounter, place the print upon it face down,

with the edges projecting all around, and on it, to hold it in position temporarily, place the paddle, then prepare the brush with as much paste as needed, after which place the rubber hose in the mouth and exhaust the air; this holds the print whilst applying the paste; after the print is pasted, apply the paddle to it, so that it may be readily transferred to the mount, place one edge in its proper position on the mount, and with the left hand hold it there, until you remove the paddle, after which proceed as in the older method.

ANNUAL REPORT OF THE DIRECTOR OF THE DRAWING SCHOOL OF THE FRANKLIN INSTITUTE FOR THE SESSIONS 1899–1900.

This has been a very successful year with the drawing school, not only as to the number of students, but also as to their character. The general prosperity of the industries of the city has had much to do with this result, but, in addition, the fact is slowly becoming known that a definite, progressive, logical and comprehensive course of instruction is pursued here, which gives a maximum of efficiency with a minimum of time and labor. There is given in our school just such a blending of the science and practice of the art as to make the study interesting and effective, and at the same time desirable, to students whose previous knowledge and experience are very different. The college student, proficient in geometry, can see how the problems that bored him so in the abstract are usefully applied, the apprentice in the draughting room can learn the principles underlying the work that he is doing in detailing and tracing the drawings of his seniors, and the foreman or mechanic can acquire the skill and technique to enable him to make his own drawings and to readily and accurately read and understand those to which he is obliged to work. The requirements necessary to obtain and maintain a position are constantly increasing, and in the mechanic arts a considerable knowledge of draughting has become a necessity. This school effectually supplies that knowledge.

WM. H. THORNE, *Director.*

THE FOLLOWING STUDENTS ARE ENTITLED TO HONORABLE MENTION:

In the Senior Mechanical Class.

John Oldman,
Samuel Greenlee,
George F. Eisenhardt,
Emil Paul Hottinger,
Fritz H. Larsen.

In the Intermediate Mechanical Classes.

Clarence Fithian,
C. B. F. Waller,
Edward Flegel,
John Hoffman,
Walter Topliff,
J. Benjamin De Hart,
Lionel F. Levy,
C. E. Moldrup,
John Vallely.

In the Junior Mechanical Class.

Richard Poeckert, Thomas F. Jones,
Joseph Roberts, Eugene Fischer,
John Zerbe, Robert McAlees, Jr.

In the Architectural Class.

Roy E. Blithe, J. Henry Slaugh,
Harry C. Beatty, Daniel Sharp,
J. De Frehn.

In the Free Hand Class.

Robert F. Plum, James J. Dunn,
Clarence Bellet.

THE FOLLOWING STUDENTS ARE AWARDED SCHOLARSHIPS FROM THE B. H. BARTOL FUND, ENTITLING THEM TO TICKETS FOR THE NEXT TERM:

Frederick Schwartz, Theodore R. Johnson,
Frederick Woodward, James R. Calhoun,
A. D. Thomas, Alfred Whitney.

THE FOLLOWING STUDENTS, HAVING ATTENDED A FULL COURSE OF FOUR TERMS WITH SATISFACTORY RESULTS, ARE AWARDED CERTIFICATES:

William Welch, Samuel Greenlee,
John A. Proud, George F. Eisenhardt,
Herman A. Langefeld, Robert Koeberle,
Joseph F. Klinger, Arthur Schwab,
John Oldman, Frederick Rieman,
Fritz H. Larsen, William H. Hollar, Jr.,
John W. Manogue, Roy E. Blithe,
George Cummings, William H. Gould,
William Williams, Robert F. Plum.

Branch School.

Johan Olof Hofman.

CORRESPONDENCE.

FALLACIES OF TEXT-BOOKS.

To the Editor of the Franklin Institute Journal:

It was in the columns of this *Journal*, if my memory serves me correctly, that the attention of the engineering fraternity was called to the error in many of the text-books relative to the loss of head by a current of water in a pipe, when the current underwent a deflection, many of the text-books showing the angle to be that made by the intersection of the two lines of pipe instead of the angle of deflection.

It was also in the columns of this *Journal* that the attention of engineers was

called at an early date to the fact that the exhaust edges of a slide valve had considerably to do with the economy of a steam engine and that "lead" was detrimental unless accompanied by suitable compression.

More recently the fallacy of the so-called "law of partial pressures" has been criticised and exploded, and a more rational base of the volumes substituted.

I desire now to call the attention of those who are interested to other fallacies, which, the sooner they are exploded and obliterated, the sooner a solid foundation will be secured, and new methods and means formulated by which and upon which engineering calculations may be more safely based for future work.

It is only necessary to refer to almost any text-book, technical, periodical or trade journal to find elaborate calculations based largely, if not entirely, upon the specific heats of various fluids (and especially of the gaseous ones), in which the specific heats, regardless of temperature or pressure, are treated as constants. The experiments and investigations of such careful scientists as Wroblewski, Olszewski, Witkowski, Amagat, Mollier and Lussana have thoroughly exploded this fallacy and have clearly demonstrated that the specific heats of many, if not all, of the gaseous fluids increase with both the temperature and pressure in many cases several hundred per cent. Moreover, it is at least suggestive, if not probable, that in most cases these increments of specific heats are additive when both temperature and pressure are changed from normal conditions.

Again, more recent investigations by scientists have thrown such serious doubts upon what has heretofore been known as "absolute zero" as to make this quantity in practical calculations not only absolutely useless, but exceedingly misleading; so much so, that I sincerely question whether any scientists at the present day, who are familiar with the present aspect of the subject, would feel warranted in stating positively that there is such a point as "absolute zero," and if there is such a point, whether or not it is within several hundred degrees of its present assumed position on the thermometric scale. These are all questions of the most serious importance, not only to the scientist, but particularly to the practical constructing engineer, who may be engaged in the construction of new machinery, involving the compression and liquefaction, expanding and vaporizing of gases for various purposes, such as refrigeration and the production and distribution of power by compressed air or other gases. And while I am not particularly anxious to raise the questions relative to the second law of thermodynamics so ably discussed in the columns of this *Journal* many years ago by Professors Klein, Thurston and others, the engineer cannot ignore the facts brought out by more recent investigators and the bearing that these facts have upon this matter. Moreover, it seems almost like a "thermodynamic heresy" to suggest anything that would invalidate the practical utility of the beautiful entropy or theta-phi diagrams, so ably treated by Professors Boulvin, Reeve, Golding, Richmond and others. However, facts are no respecters of either persons or things, and if it *is* true that the specific heats of gases change materially by variation from the normal, of either temperature or pressure, or both, and if it *is* a fact either that there is no such a condition as that heretofore conceived as "absolute zero," all of the beautiful formulas so elaborately set forth in the text-books

relative to adiabatic compression and expansion, and in fact all other formulas or calculations of any kind based upon the alleged "absolute zero," must either be entirely eliminated or so modified as to meet the new conditions, in place of being based upon the former erroneous assumptions.

Without wishing to influence the opinions or actions of others in the premises, my own conclusions based upon recent researches and investigations are that the larger part, if not all, of the thermodynamic formulas, as appear in the text-books, are not only untrue, but absolutely misleading; that the point on the thermometric scale known as absolute zero either has no existence in point of fact (which is the most probable), or if there is such a point, it is so far below its preconceived position that it cannot be used in engineering calculations to any extent whatsoever for practical purposes; that the change in the specific heats of gases, by variation in temperatures and pressures, are so great that these values are also not only useless, but misleading, to the engineering profession, when they are made use of in calculations upon which to construct machinery for practical purposes; that there is not a sharp line of demarkation between the liquid and gaseous condition of the matter, but that the lower the temperature, the sharper the line, the higher the temperature, the less distinct becomes the line of demarkation until the critical point is reached; that, with fluids, the critical temperature of which is above that of the normal temperature, the specific heat of the fluid in gaseous state increases both with an increase of temperature and pressure until the critical state is reached, at which point the specific heat then becomes that due to the fluid, at that point, and that the change in specific heat from one state to the other is gradual.

I am aware that these ideas may be, and probably will be, called "thermodynamic heresies," and that in ancient times people have been put to death for the expression of less radical ideas than these; however, I annunciate them for what they are worth, and expect that if the same are not ignored by those who are better able to discuss them than I am, to be severely criticised. I am simply seeking the facts, and will certainly have no unpleasant feelings toward any one who differs with me in the premises, and only the most pleasant feelings toward those who may feel disposed to show me that I am in the wrong.

E. F. OSBORNE.

CHICAGO, ILL., April, 1900.

NOTES AND COMMENTS.

THE PASSING OF THE LINK AND PIN.

A branch of industry which will, ere long, become extinct, is the manufacture of links and pins for coupling railroad cars. Automatic coupling devices render unnecessary the use of these ancient contrivances for fastening cars together. The adoption of automatic couplers has been hastened by national legislation, even though the time has been considerably extended before the penalties are imposed, and years have passed since the Congressional enactment first went into effect without the punishment of any railroad official for tardiness in making the improvement. So much progress has been shown

in changing the equipment of cars that the day is undoubtedly near at hand when links and pins will only be seen in collections of railroad curios. Railroad supply houses which once handled them in carload lots, and kept large stocks to meet the steady demand, now find their orders calling for a few at a time, while such orders are likely to be far apart. The passing of the link and pin is simply another illustration of the march of progress. Apart from the danger to railroad operatives, the old-fashioned method of coupling cars is objectionable as applied to modern rolling stock, and would probably have been supplanted in due course by the automatic coupler, even without a compulsory law.—*Iron Age.*

DISSOCIATION OF AIR AT ORDINARY PRESSURE.

Prof. Raoul Pictet, of Geneva, Switzerland, whose name is identified with the early liquefaction of air and gases, recently made a demonstration, says the *New York Sun*, of a process of his invention for the separation of the oxygen and nitrogen of the air at ordinary pressure.

It is said that the process is one that is about to be introduced in this city on a commercial scale. Professor Pictet's process as described consists in the initial production of a certain quantity of liquid air which is stored in tubes. Then through this is forced, under a pressure of only about one atmosphere or 15 pounds to the inch, a stream of atmospheric air. This is cooled in the liquid air, but as it rises in a chamber beyond the gases of which it is composed separate themselves by gravity and run off in separate tubes. The oxygen, being slightly the heavier, flows out through the lower tube, while the nitrogen goes off above. In addition to these gases, the air contains as an impurity carbonic acid gas, and this, it is asserted, leaves the machine in a liquid form, being reduced to that form by the low temperature. In ordinary liquid air as it is produced by Tripler, Ostergren and others, the carbonic acid gas is frozen and gives the liquid air a milky appearance. It is taken out by pouring the liquid air through an ordinary paper filter.

In a demonstration recently the apparatus used was of the laboratory character, and the proof of the effect was made by exposing a burning bunch of tow to the end of the pipe whence oxygen was expected to flow, where the combustion was made more intense, while at the end of the other pipe the neutral nitrogen diminished or extinguished the flame.

In the commercial machine it is promised that with an expenditure of 500 horse-power the daily output will be 500,000 cubic feet of oxygen, ranging from 50 to 90 per cent. in purity, and 1,000,000 cubic feet or more of nitrogen of similar quality. In addition, it is promised that 1,500 pounds of liquid carbonic acid will be produced.

The two products for which a direct commercial use is expected to be found are the oxygen and the liquid carbonic acid gas. The latter already has a fixed place in the market and large quantities of it are saved in well-equipped breweries, where it is produced in great bulk through the fermenting of beer. It is pumped into steel tubes under a pressure that liquefies it. It is worth about 7½ cents a pound.

The great market which Professor Pictet expects to find for the oxygen is to support combustion at high temperatures in furnaces where coal is burned,

making such fires available for purposes which only the electric arc is now suitable for, as well as making a great economy in producing heat for ordinary purposes. In burning fuel with the oxygen of the air there must be admitted to the furnace about three times the bulk of oxygen or nitrogen, and this absorbs a large quantity of the heat. If an excess of air goes into the furnace, this also takes up and wastes heat. By admitting oxygen these losses can be saved. This saving, Professor Pictet thinks, would equal 40 per cent. of the present fuel bill.

It is proposed to put the oxygen in tubes or tank cars and ship it to consumers. Of the theoretical value of it there can be no doubt. The commercial feature remains to be demonstrated. The nitrogen, it is asserted, can be used for the production of nitric acid, and Professor Pictet says that by a process of his invention he can combine it into ammonia directly by exposing hydrogen and nitrogen to the electric arc under certain conditions. If this be true, Professor Pictet has solved a problem of wonderful value which has defied the researches of the ablest chemists of the world.

Concerning the foregoing statements there would appear to be reasonable probability that Professor Pictet has succeeded in practically solving the important problem of separating the constituents of the atmosphere, which in itself is an important accomplishment. The fixation of the atmospheric nitrogen in the form of ammonia, the heat of the electric arc, has repeatedly been attempted and without success. W.

BOOK NOTICES.

La Telegraphie sans Fils. Par André Broca, Professeur de Physique à la Faculté de Medecine. Paris: Gauthier-Villars. 1899. 16mo, pp. 262, with 34 figures in the text. (Price, 3.50 francs.)

This work is intended to serve the convenience of those who, although not specialists, are interested in the most recent advances of science, and who desire to keep themselves *au courant* with its applications. W.

Les Recettes du Distillateur. Par Ed. Fierz. 16mo, pp. 149. Paris: Gauthier-Villars. 1899. (Price, 2.75 francs.)

This volume contains 140 receipts for the preparation of divers liqueurs, crèmes, etc., concluding with instructions respecting the composition of various coloring matters indispensable to the distiller. W.

Small Engines and Boilers. A manual of concise and specific directions for the construction of small steam engines and boilers of modern types, from 5 horse-power down to model sizes. By Egbert P. Watson. Illustrated by thirty full-page working dimensional drawings. New York: D. Van Nostrand & Co. 1899. (Price, $1.25.)

This work has been prepared for the special benefit of amateur and other non-professional workers who need a guide for the construction of small engines and boilers, and who are presumed to have some acquaintance with

ordinary machine work, and some facilities for doing such work. The author has made his instructions very clear, and, with the aid of his dimensioned plans, the book should prove very useful to the class for which it has been prepared. W.

Photographic Mosaics. An annual record of photographic progress. Edited by Edward L. Wilson, Editor of "Wilson's Photographic Magazine," etc. Thirty-sixth year. 12mo. New York: Edward L. Wilson. London: Dawborn & Ward, Ltd. 1900.

The edition of this well-known photographic annual has retained in its thirty-sixth volume the general features with which its army of readers are familiar.

It embraces a résumé of photographic progress in 1899, covering every branch of the art; and a section of original contributions from well-known experts; the whole covering 288 pages, and embellished with a number of pictures from the best professional workers. W.

Compend of Mechanical Refrigeration; a comprehensive digest of applied energetics and thermodynamics for the practical use of ice manufacturers, cold storage men, contractors, engineers, brewers, packers and others interested in the application of refrigeration. Third edition. By J. E. Siebel, Director Zymotechnic Institute, Chicago. Chicago: H. S. Rich & Co., 1899. 12mo, pp. 389. (Price, in cloth, $3.00; in flexible morocco, $3.50.)

The author appears to have condensed in this small volume a large amount of information, theoretical and practical, bearing upon the subject of refrigeration and its numerous practical applications in the arts. It should prove of special utility to engineers and others who are directly interested in the use of refrigerating machinery, since the author, in addition to devoting special chapters to the practical application of refrigeration, has collected and presented in convenient form a large body of rules, tables and formulæ which will be found of much value for readily answering many questions that arise in the practice of the art. W.

Victor von Richter's Organic Chemistry; or chemistry of the carbon compounds. Edited by Prof. R. Auschütz (assisted by Dr. G. Schroeter). Authorized translation by Edgar F. Smith, Professor of Chemistry, University of Pennsylvania. Third American from the eighth German edition. Volume II. Carbocyclic and Heterocyclic Series. Philadelphia: P. Blakiston's Son & Co. 1900. (Price, $3.00.)

The present edition of this admirable work needs no special endorsement to commend it to students and teachers. It embodies the latest theoretical and experimental developments of this branch of the science of chemistry. The translator's share in the work has been thoroughly well done, and the publishers have issued it in their customary creditable manner. W.

Some Properties of Rocks and Soils in Relation to the Search for Potable Water and the Perforation of Tunnels. By Prof. Gustavo Uzielli.

The conclusions reached in this pamphlet, especially those regarding the expansion of soils due to air chambers formed by the filtration of water and

the contraction which these chambers produce upon tunnels in process of construction, are of considerable importance in the application of geology to engineering. The experiments performed by the author to show the formation of air chambers by water filtration through various kinds of soil are very interesting and convincing. The work throughout is an exhibition of profound thought and clearness, and should be brought to the notice of every engineer interested in geological studies. L. D'A.

Franklin Institute.

[*Proceedings of the stated meeting held Wednesday, April 18, 1900.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, April 18, 1900.

MR. JOHN BIRKINBINE, President, in the chair.

Present, 96 members and visitors.

Additions to membership since last month, 29.

The Actuary's report contained the following reference: "The Board * * announces, with extreme regret, the decease of the Senior Vice-President of the Institute, Mr. Chas. Bullock, who died March 21, 1900." The President stated, in connection therewith, that the Board, in special session, had adopted formal resolutions of respect to the memory of its deceased member and officer, and that a committee had been appointed to prepare a suitable memorial for publication in the *Journal*.

A special election was held to fill the vacancy in the vice-presidency caused by the death of Mr. Bullock, and resulted in the election of Mr. Washington Jones.

The vacancy in the Board caused by the election of Mr. Jones to the vice-presidency was filled by the election of Mr. Samuel F. Houston.

Dr. Joseph W. Richards, of Lehigh University, Bethlehem, Pa., presented an interesting communication on "Recent Progress in the Aluminum Industries."

Dr. Richards referred more particularly to the steadily increasing use of the metal for the production of a great variety of decorative articles; for electrical conductors in place of copper; for culinary vessels; for lithographic work; and, in the form of powder, as a flash-light in photography; as a paint for protecting exterior iron work, and for the reduction of the oxides of the difficultly reducible metallic oxides, such as chromium, manganese, titanium, vanadium, etc. The speaker illustrated his remarks by the exhibition of a large collection of specimens, and performed the interesting experiment of reducing chromic oxide with powdered aluminum.

Mr. S. Ashton Hand made some most instructive remarks on the "Development of Negatives," giving the results of his long experience in the art of correcting defects in negatives due to over-exposure, under-exposure and other defects familiar to photographers. Mr. Hand illustrated his remarks by the

exhibition of a series of negative and positive lantern slides of the same subject, in which these various defects and their proper treatment were shown in a most instructive way.

Mr. Chas. P. Jacobs described and exhibited an improved apparatus for aërating beverages, which involved the employment of small steel capsules filled with liquid carbonic acid.

The Secretary made some comments on the recently devised process of Mr. E. G. Acheson for converting anthracite culm into graphite, and exhibited a number of samples of the product, which, according to analyses, was almost chemically pure. The process of Mr. Acheson's method is a development of his electric furnace process for producing carborundum.

Messrs. Wm. R. Webster and Carl Herring were appointed delegates of the Institute to the various Scientific and Technical Congresses to be held in connection with the Paris Exposition.

Adjourned.

WM. H. WAHL,
Secretary.

COMMITTEE ON SCIENCE AND THE ARTS.

[*Abstract of proceedings of the stated meeting held Wednesday, April 4, 1900.*]

MR. H. R. HEYL in the chair.

The following reports were adopted:

Rail Joint.—Harry Villenoweth, Philadelphia.

ABSTRACT.—The invention is the subject of letters-patent of the United States, No. 574,466, dated January 5, 1897, granted to applicant, and is designed to effectively hold the contiguous rails in alignment and prevent their longitudinal displacement; also, to take the place of the ordinary fish-plates by substituting a brace on each side of the rail, extending beneath the rail. After examination, the sub-committee charged with the investigation reported that the device contained too many parts for any joint to work properly at the connecting ends of rails where expansion and contraction must be provided for and vertical stiffness obtained; also, that there are numerous other joints in use which come nearer to meeting the requirements of railway service than the invention under consideration. Certain specific mechanical objections are also referred to in detail. [*Sub-Committee*, J. J. DeKinder, Chairman; Jos. T. Richards.]

Round-lap Baling System for Cotton.—American Cotton Co., New York.

ABSTRACT.—By the system and machinery employed by the American Cotton Company a product is made which is known commercially as "Round-lap Bale Cotton," which is said to have advantages possessed by no other bale in the market. For example, each and every bale can be unrolled clear to the center, and, as they are unwrapped, can be fed to the lappers without extra hand labor, and several bales can be unrolled and fed into the lapper at the same time, thus mixing automatically. After specifically pointing out these advantages, the report concludes as follows:

"From a great mass of evidence which the Sub-Committee has gathered, it appears that the Round-lap Bale System introduced by the American Cotton Company has positively done away with all the objectionable features of the old-style 'square-bale' method of baling cotton, against which the cotton spinners of Europe and America have been protesting for many years; and, incidentally, that the new system has introduced large economies, the benefits of which are enjoyed by producers and factors as well as by the spinners."

The award of the Elliott Cresson Medal is made to the American Cotton Company in recognition of the importance of the revolution in the cotton industry brought about by the introduction of the "Round-lap Bale System."

Also, the award of the John Scott Legacy Premium and Medal is recommended to Magnus Swenson, for his invention of the Round-lap Baling Cotton Compress. [*Sub-Committee*, Chas. E. Ronaldson, Chairman; James Christie, M. R. Mucklé, Jr., Chas. A. Teal.]

Press for Compressing Cotton, etc.—Geo. A. Lowry, Chicago, Ill.

ABSTRACT.—This machine is equally well adapted to compress and bale cotton and all varieties of fibrous materials, coarse and fine hay and metal scrap.

In the case of cotton, the machine obviates the necessity of two distinct compressions, to which square-bale cotton is usually subjected. The machine may be erected and operated at the cotton-gin, where the cotton as it leaves the gin falls or is fed into the hopper of this compress, where it is subjected to one compression, baled, wired, put into bags and shipped to its destination and without further manipulation.

After describing in detail the mechanical features of the Lowry press (which would not be intelligible without the aid of illustrations), the report proceeds to enumerate the meritorious features of the machine as follows: These consist in (1) its adaptability to bale and handle a great variety of materials; (2) its general efficiency; (3) the uniformity and density of the finished product, and certain specific mechanical advantages which are enumerated. Together these constitute new and important features in apparatus of this class. The report dwells also on the fact that the machine is applicable to the compression of a large class of raw products.

The importance of the invention is recognized by the recommendation of the award of the Scott Legacy Premium and Medal. [*Sub-Committee*, Chas. E. Ronaldson, Chairman; M. R. Mucklé, Jr., James Christie and Chas. A. Teal.]

Improvements in Steam Injectors.—Strickland L. Kneass, Philadelphia.

This report, after discussion, was referred back to the Sub-Committee for the consideration of certain objections.

The following reports passed first reading:

Basin System.—C. L. Ricker, Newburgh, N. Y.

Letter and Document File.—Wm. H. Tucker, Newark, N. J.

Exhibit of the Pencoyd Iron Works, at the National Export Exposition.

Exhibit of A. J. Holman & Co., Philadelphia.

Exhibit of Laird, Shober & Co., Philadelphia.

Exhibit of U. S Geological Survey.

Variable-Speed Countershaft.—Milton O. Reeves, Columbus, Ind.

W.

SECTIONS.

SECTION OF PHOTOGRAPHY AND MICROSCOPY.—*Stated Meeting*, Tuesday, April 3d, 8 P.M.

This section organized by the election of the following officers: President, Dr. Henry Leffmann; Vice-Presidents, E. E. Ives and John G. Baker; Conservator, Dr. William H. Wahl; Secretary, F. M. Sawyer.

Mr. S. Ashton Hand read a paper, which was fully illustrated, on the making of lantern slides, with a series of very instructive illustrations. A communication was presented by Dr. Martin J. Wilbert on "Photographing Without the Aid of Light." Among the interesting experiments shown was one in which a coin was heated and then inclosed with a sensitive plate in such a way as to entirely exclude light, with the result of producing a negative in which the heat waves are supposed to have been the active agent.

Mr. John G. Baker exhibited on the screen some stereoscopic views by means of a special stereoscope which he had designed.

The regular meeting night was changed from the first Tuesday to the first Thursday of each month, the next meeting to take place on Thursday, May 3d.

F. W. SAWYER,
Secretary.

PHYSICAL AND ASTRONOMICAL SECTION.—*Stated Meeting*, Friday, April 6, 8 P.M. Dr. Wahl in the chair.

Mr. H. M. Watts made an informal address on "The Proper Organization of the Modern State Weather Service."

Mr. Watts reviewed the history of the various State weather services that have been organized with the co-operation of the United States Weather Bureau, devoting more especial attention to that of Pennsylvania, which, through the instrumentality of the Franklin Institute, was organized in 1857. It was one of the first and was conducted under the direction of the Committee of Meteorology of the Institute with the aid of an appropriation from the State for some eight years, when, owing to the failure of the State to make an appropriation, the work was taken up and continued by the United States Weather Bureau. Mr. Watts thought the time was now ripe for a concerted movement among the various scientific, technical and other institutions of learning of the State to take up the subject in a systematic way and build upon the present foundation a service for the State which with such co-operation could be made far more useful than it formerly was. He instanced a number of directions in which such a service might be extended with great advantage, especially to the commercial, agricultural and manufacturing interests. He cited the Maryland and Massachusetts State Weather Services as typical examples of what might be done in Pennsylvania by a properly organized and equipped establishment.

A lengthy discussion followed Mr. Watts' address, in which Mr. T. F. Townsend, Assistant in charge of the present State Weather Service; Professor Doolittle, of the University of Pennsylvania; Dr. Wahl, Secretary of the Institute, and others took part.

The subject was made a special order for the stated meeting in May.

W.

ELECTRICAL SECTION.—*Special Meeting*, held Tuesday, April 10, 8 P.M. Prof. W. S. Franklin in the chair. Present, 84 members and visitors.

Mr. Chas. F. Scott, Chief Electrician, Westinghouse Electric and Manufacturing Company, presented a paper on "Modern Central Station Practice." Mr. Scott pointed out that the word "modern" in this connection must refer to the last four or five years, as the "ancient" in central stations dates back only a score of years. The bitter controversy between alternating current and direct current which prevailed ten or twelve years ago has been renewed in another form. The direct current has had a fairly well-defined field in centers of cities, while the alternating current has not only found favor in towns and outlying districts where areas are wide, but has also been used extensively in city distribution as well.

In the new engineering which is made necessary by the extension and combination of electric interests, it is essential to generate power in large stations favorably located. Direct-current distribution therefore demands an alternating-current power station distributing to sub-stations, where the current is converted into direct current by rotary converters. The question then arises whether it is better to convert into direct current, or to distribute the alternating current without the intervening machinery with its inherent losses and the complication in operation which are involved if the current is converted into direct current. The various pieces of apparatus, including generators, rotary converters and motors, which are used in connection with the central station system were then taken up in detail. The most recent types of construction were illustrated and explained, and particular attention was given to those characteristics which are of particular engineering interest, and which bear particularly upon the choice between direct current and alternating current distribution.

In connection with the parallel operation of alternators, a mechanical analogue was presented in the form of two engines, which drive a common load through gear wheels which are connected with pinions on a common shaft, instead of alternators which are connected to common bus bars. It was pointed out that the operation of the engines, both as to the manner in which they may be first connected together, the conditions relative to division of load and to the shifting of load between the two engines are the same in most respects whether the engines be connected by gear wheels or alternators.

Some interesting facts respecting the operation of rotaries, particularly as to "hunting," were presented, together with the means which are effective in counteracting the various tendencies to this action. It was stated that the better class of rotaries are now operating with the same high standard of excellence which characterizes other types of electrical apparatus.

The old controversy between the induction and the synchronous motor is rapidly dying out, as there are few, if any, who advocate the synchronous motor for small work, while the dividing line in size between the field where induction motors are universally accepted and that in which there is still some difference of opinion is rising. Mr. Scott stated that he thought it quite certain that the induction motor would not wholly replace the synchronous motor, as there are some few conditions under which some of its features are of sufficient value to more than compensate for its many objectionable

elements. The adaptability of the induction motor to general service of a city where simplicity and reliability are of paramount importance was explained, and it was pointed out that while reliability rather than efficiency is the important element, yet the efficiency of the induction motor is practically the same as that of the direct-current motor at full load; it is higher at small loads, particularly in small sizes, where the brush friction of direct-current motors is comparatively high.

The characteristics of alternating-current apparatus make it in many respects the ideal form for use throughout the whole of a city plant. The difficulty which confronts the engineer at the present moment is the necessity of using the various existing apparatus in a comprehensive system, while at the same time he may be working forward to ultimately reach the system which will possess the greatest simplicity, reliability and economy in its operation.

The meeting passed a vote of thanks to the speaker of the evening and adjourned. W.

MINING AND METALLURGICAL SECTION.—*Stated Meeting*, held Wednesday evening, April 11th. Mr. Joseph Richards in the chair.

Mr. Wm. R. Webster, Chairman of Committee No. 1 of the American Branch of the International Association for Testing Technical Materials, presented a report on the work that had been accomplished by this Committee. Advance copies of the specifications drawn up by the Committee are ready for those desiring to enter into the discussion of these in the fall.

Mr. Robert Job, Chemist of the Reading Railway, Reading, Pa., read a paper on "Railway Journal Bearings." He emphasized the importance of the proper structural arrangement of the metals in the composition, gave some good hints on the proper manipulation of this class of material in the foundry and described the causes prevalent in causing the so-called "hot box." The paper was well illustrated by a number of photographs and photomicrographs. The paper was discussed by Mr. Squire, of the Atchison, Topeka & Santa Fé R. R.; Mr. Brown, of the B. & O.; Mr. G. H. Clamer, the author, and others.

A vote of thanks was tendered Mr. Job for his interesting paper.

Adjourned.

G. H. CLAMER,
Secretary.

MECHANICAL AND ENGINEERING SECTION.—*Stated Meeting*, held Thursday, April 12th, 8 P.M. Prof. J. F. Rowland, Jr., in the chair.

Present, twenty-four members and visitors.

The topic for discussion, "Power Transmission by Belting, Ropes and Chains," was opened by Mr. Wilfred Lewis, who gave a general *résumé* of the subject, including the results of his own experiments on transmission by belting. He was followed by Mr. F. Barth, of Bethlehem, Pa., who entered more closely into the theory of leather belting, as deduced from practice in large machine shops and factories. Messrs. Spencer, Miller and Fullerton closed the discussion with a few remarks in reference particularly to rope drives and driving, and the proper designing of sheaves for that purpose.

D. EPPELSHEIMER, JR.,
Secretary.